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## EDITORIAL NOTE

THIS number of our *Journal* is devoted to commemorating the Bicentenary of George Berkeley (12th March 1686–14th January 1753). We do so to recall his outstanding contributions to both science and the philosophy of science, which have been accorded, we believe, insufficient recognition. It is not within our province to deal with his metaphysics, and the papers in this number—apart from a sketch of his life—confine themselves to discussing Berkeley's contributions to science and its philosophy. The papers are, incidentally, of very different length, but this in no way reflects our judgment of the relative merit of his work in different fields.

We have not included any discussion of Berkeley's interest in natural history, because his contributions do not seem to us of sufficient significance. Likewise we have omitted to discuss his voluminous work on tar-water; for, though *Siris* is one of the strangest books ever written by a philosopher, and Berkeley was by no means senile—as has been suggested—when he wrote it, we do not consider that it makes any contribution to knowledge.

His work in our field comes under four headings: (1) His psychology of visual perception: in this he attempted to explain visual judgments by associations with tactual experience. This is regarded by some psychologists as a classic. (2) His criticism of Newton's concept of the infinitesimal: this was highly original and had a marked influence on subsequent mathematics; it was a forceful logical criticism of a subject that loses its *raison d'être* if it is not logical. (3) His view of the nature of Newtonian physical concepts: this contains a most striking anticipation of Mach's criticism of absolute space. (4) His economics: this, with its strong Keynesian flavour, shows a clear grasp of essentials and a considerable power of application to concrete problems.

We offer the present number on these four contributions in the belief that they have a living interest.

## BERKELEY AND MODERN PSYCHOLOGY

E. TRANEKJÆR RASMUSSEN

WHEN, as a psychologist with special interest in the psychology of perception, I read Berkeley's main works, I was struck time and again by the relevance for present-day psychology not only of a number of his main points, his way of thinking and speaking about things, but also of many of his more detailed statements. It is very easy for the writings of a famous philosopher of the past to be pigeon-holed, summarised and judged as exceptionally interesting, a work of genius, but nevertheless, in the opinion of a large number, mistaken. This can be misleading because between the lines of his works is often found an attitude of mind, an atmosphere which, though difficult to characterise, has perhaps exerted greater influence than the traditionally quoted results. Berkeley is, I believe, a case in point. Let me, in what follows, try through some examples to give an impression of the vitality of a certain basic attitude of his, and of the importance it can still have for modern empirical psychology.

The *New Theory of Vision* must presumably be the work that first catches the eye of a psychologist. His considerations about 'abstract ideas', and in particular his assertion that it is meaningless to speak of anything unless this 'something' is an object of consciousness (i.e. exists in consciousness) will also attract attention. These subjects are closely interconnected in Berkeley's philosophy. In this article we will look at them mainly from the standpoint of psychology, beginning with the postulate that every entity must be regarded as a 'something' that is an object in a consciousness.

In modern perception-psychology it is becoming more and more clear that one of the most important methods of investigation is that of phenomenological description, in other words, the description of what we experience as opposed to what we otherwise could say is present or exists in a different sense, for example, in a physical way where we deal with the physical stimuli affecting the sensory organs. This leads to a sharp distinction between the experienced and the physically present entities, even though both are often called by the same name in everyday speech. There is a difference between perceived red and what the physicist calls red light, which consists of electromagnetic waves that can stimulate the eye and give rise to nerve impulses that are processed by the central nervous system into per-



## BERKELEY AND MODERN PSYCHOLOGY

ceived red. There is a difference between what the physicist calls a straight line and the appearance of straightness, which does not enter into the physicist's definition of a straight line. There is a difference between the experienced tone and the physical tone, which consists of air waves which stimulate the ear and cause nerve impulses that are processed into the experienced tone. There is a difference between perceived space and physical space, perceived time and physical time, in fact even differences between experienced number and number in a physical sense, as is seen in Ponzo's investigations. There is a difference between everyday reality as experienced by us and the reality or objectivity with which the physicist works: if this seems difficult to accept, we can point to how, in a high fever, we may experience the surrounding world as in a sense unreal. The physical world is not present, does not 'exist', for the man in the street, but psychologists are interested in discovering how the physical stimuli are processed into experience. If we know enough about the structure of the experiences and are well acquainted with the physical stimulus situation, it is often possible to say something about the structure of those processes in the central nervous system responsible for the resulting experiences. But in order to gain knowledge about the experienced situation we must hold ourselves to what is present in consciousness, must describe the 'phenomenological' situation as we say; if we *know* something about what is present physically, we must try to forget it, and it is often best to use subjects without training in physics. It is often more difficult than we might believe to hold ourselves to the 'experienced' or 'phenomenological' entities alone, but if we read through Berkeley's works we find time and time again passages where he energetically demands that the reader hold himself to what he experiences, passages that could not be better formulated by a modern psychologist giving instructions to his subjects. Perhaps he had a different goal, namely to prove a philosophical postulate, but this does not detract from the value of his basic attitude as a pioneer of modern psychology. We cannot help admiring his constant appeal to experience as experience, and, in the light of this, his continuous demands for the revision of the meaning of words in order to cast out the meaningless or the uncontrolled contents, his firm acceptance of the everyday phenomenological world, and his commonsense attitude in the ensuing battle against many philosophical speculations. We might well be jealous of his firmness in this respect, and wish that it was more common among many modern

psychologists who, though they recognise the phenomenological method in principle, all too often do not put it into practice so untiringly as Berkeley.

But what is the quality of Berkeley's phenomenological descriptions measured by modern phenomenological-psychological standards? We are often amazed by surprisingly good descriptions, but without doubt we also meet much that a modern psychologist would not accept. But how could it be otherwise? How could he, a pioneer in this field, reach what numerous other experimental workers have attained only by systematic co-operation and by extensive investigations? It would be easy to point out various gaps in his descriptions. For example, in his descriptions he tacitly accepts without further analysis what in modern psychological terminology would be called the mosaic psychological point of view.<sup>1</sup> A perceived object, for example, is nothing else but the sum of a number of perceived sensory elements. 'Thus, for example, a certain colour, taste, smell, figure and consistence having been observed to go together, are accounted one distinct thing, signified by the name *apple*'.<sup>2</sup> Occasionally his phenomenological descriptions are apparently not restricted by this point of view, which presumably is related to the fact that Berkeley sometimes uses the term 'sensory elements' in a wider sense than the later mosaic psychology does. The so-called psychology of wholes, under which come the 'Gestalt' psychologists, has recently shown that we experience things as wholes that cannot be regarded as merely integrations of sensory elements. What were previously believed to be experienced as isolated sensory data are in fact experienced, if they are experienced at all, as aspects of experienced wholes. If we take this into consideration, several of Berkeley's postulates must be formulated in another way, but many of his main statements would retain their most important contents because the experienced whole is actually a *sensory* experience and not intellectually presumed or the result of philosophical speculations. In the case of the apple which was mentioned before, the apple as a whole is just as directly experienced as the other data 'colour, taste, smell, figure and consistence'. His 'abstract ideas' can be regarded in a similar way, where, although he is most likely incorrect seen from

<sup>1</sup> Concerning the so-called mosaic points of view, the reader can be referred, e.g., to : Köhler, *Gestalt Psychology*, 1947.

<sup>2</sup> Berkeley, *A Treatise Concerning the Principles of Human Knowledge*, Part I, Section I



## BERKELEY AND MODERN PSYCHOLOGY

a modern phenomenological point of view, and although, as we will see shortly, we must formulate his statement in another way, this will not weaken his use of the denial of the existence of 'abstract ideas'.

A philosophically inclined reader of this article will perhaps soon ask impatiently: 'What is the opinion of psychologists concerning Berkeley's main thesis that it is meaningless to speak about something that does not exist in consciousness? We understand that psychologists do not accept so-called naïve realism, with its acceptance of the everyday world regardless of the processes of our central nervous system; we understand that we must consider this outer world with all its things, qualities, relations, states, etc., as the result of processing in the central nervous system of physical stimuli that activate the perceptual organs. We understand also that in this connection it can be fruitful to hold ourselves to what is present in consciousness, as did Berkeley; in other words, to carry out the phenomenological programme. But to go from this to Berkeley's main postulate is a big step. Psychologists do, after all, accept the physical world and the physical stimuli as existing independently of consciousness, and this would then contradict Berkeley's thesis.'

I am not quite sure how many psychologists will agree with my answer. Even if I answer only on my own behalf I believe that my reply will reflect a tendency in psychological work today, and in any case, a possible and logical development. Let me try anyway.

Just as we investigate the everyday world phenomenologically (that is to say, true to the spirit of Berkeley), we can examine the physical world phenomenologically using as subjects the small group of persons for whom the so-called physical world 'exists'. We *must*, in fact, do so if we are to know anything about the psychological conditions determining how it comes about that this physical world can 'exist' for them. We can ask the physicist: What do you experience when you stand in your laboratory and make your observations? Describe accurately what you experience when the one or the other *physical* thing presents itself to you and is observed by you, or when one or another *physical* relation is present for you and is observed by you. Describe in detail the 'experienced' physical entity even though it is not experienced as directly as everyday entities. What do you *experience* when one or another thing is experienced as existing for you in a physical way? We can thus examine the matter in the same way as did Berkeley—as, in fact, we should do. For some time (presuming that physics continues to make progress

as it has done hitherto) all these physical entities of today will not 'exist' for anyone, just as phlogiston, for example, does not 'exist' for anyone today; or they will be regarded as existing only in consciousness, like, for example, Newtonian time or space, which is today regarded as a subjective construction, useful when employed on certain phenomena, whereas space and time as described by Einstein are more 'real' for us. Knowledge, as was stressed by the Danish philosopher Høffding, is incompletable and there is therefore no guarantee for the eternal existence of physical things in our consciousness. If they are no longer part of present-day physical science, we may smile tolerantly when they are mentioned as, for example, we do at phlogiston, and say: Ah, they only 'existed' in the minds of physicists of that time! If we want to retain the objects of physics (things, qualities, relations, states, etc.), we must do so by describing them phenomenologically as they are (when they are) in consciousness. In other words, when we speak about the colour red as it is in everyday life, we do not mean the electromagnetic waves that stimulate the eye and are processed into experienced red, but the red colour that is *seen* in everyday life. If we speak about electromagnetic waves we mean that which is electromagnetic waves for the physicist today and *not* that which a physicist may in 2,000 years say was *actually* present. If this point of view is carried over to epistemology, it will mean that we must examine a number of progressive stages within our knowledge where, in passing from stage to stage, we change our views about what we regard as objective and subjective;<sup>1</sup> or, to put the point in another way, as we go from stage to stage we develop new views about objectivity or reality compared with which the presumed objectivity of the earlier objective entities are now regarded as having had their existence only in consciousness.

Scientists working with entities from the highest stage yet reached will most likely be inclined to believe that the objectivity with which they are working is absolute, and this may perhaps be necessary for many of them. But I cannot see that the modesty and respect which must necessarily be the result of casting a glance into the future, and thinking about the as-yet-unknown objectivity we sometime or other will have to work with, can harm or hinder us. On the contrary, it

<sup>1</sup> In a work that is unfortunately as yet unpublished I have discussed this point of view in more detail. A resumé can be found in *Gesetz und Wirklichkeit*.—*Internationale Hochschulwochen des österreichischen College, Alpbach-Tirol, 21 August bis 9 September 1948, Innsbruck, 1949.*



## BERKELEY AND MODERN PSYCHOLOGY

will in fact be connected to part of what is most noble in a scientist, namely his wish that human knowledge may continue to develop, or, in other words, his wish that all the things that come to his scientific knowledge will later show themselves to exist only in consciousness. This point of view asserts that although mankind will continue to strive to attain an ever more firm objectivity, and indeed must do so, more and more will finally be considered as existing only in consciousness, in spite of many not realising it. In one way we may say that knowledge of what exists objectively will always be relative, while knowledge of what exists subjectively will be absolute. Knowledge about 'outer' nature will always be relative and changing, while knowledge about the nature of conscious life in that way will be definite, as all things in our knowledge will sooner or later end by existing only in consciousness presuming that the science always makes progress. We could also say that the objectivity of knowledge is perhaps always only a necessary transitive phase, but never anything absolute. Our knowledge of the nature of mental life gains a more and more extensive character with the advance of science, and the psychologist is put more and more questions; he is asked to explain the mental processes that give rise to this advance. Psychological knowledge, like other fields of knowledge, will also, in consequence of what has already been said, be incompletable. We shall never reach a final and complete body of knowledge concerning human mental life, or even be able to imagine a final and complete picture of human mental life. Nevertheless, a scientist usually considers, and must consider, his investigations as composing part of a gradual advance, where what is unknown 'lies ahead' of him. If he dares to try to imagine the whole of the yet unknown world as a completed whole, and if he seriously puts into practice the considerations outlined above, he must also imagine the unknown world as a whole existing only in one or another consciousness, of which he can form no final picture but only approximate pictures, namely the portions of knowledge about human mental life that have been formulated up to the present. We could now understand if he felt a desire to say that all the as-yet-unknown world, as a completed whole, could exist only in the consciousness of an eternal mind or in the mind of God, as did Berkeley. Nevertheless, it is reasonable to warn others against trying to include all this unknown world as a completed whole in one act of imagination, without further ado; we ought, perhaps, to do so only as part of religious or artistic inspiration.

If, on the basis of contemporary psychology, I am right in extrapolating as I have in the above (which unfortunately can only be formed as vague outlines), it can be seen that Berkeley's main thoughts not only are of present interest today, but in the future may attain even greater actuality for psychology and its epistemological basis.

But let us return to the more concrete psychological questions and look a little more closely at Berkeley's denial of the existence of what he calls 'abstract ideas'. With regard to this point I believe that modern psychologists will agree with me that Berkeley was wrong, from a phenomenological point of view, when he formulated his postulate as he did and used the examples given. If I draw some triangles on a blackboard, some obtuse and some acute or right-angled, and ask the subjects to describe what they see, many will say that they 'see' something common in all the figures, something 'triangular' perhaps. This quality is present at different parts of the board simultaneously, almost in the same way as the colour of the board is present in several places at once. Some subjects see this more directly than others, and can in some way retain this common quality of triangularity as something separate and regard the equilateralness, obtuseness, acuteness, etc. as the 'surroundings' (in a certain sense of the word) of the common quality of triangularity. If I look at something, my fountain-pen for example, it is impossible for me to perceive it without at the same time perceiving certain surroundings, for example my desk, jacket, books, paper, etc.; but in this case it is easier to separate things from what we would call their 'surroundings'. A housewife recognises a table-cloth irrespective of whether it is covering a round or a square table, whether it is folded or merely crumpled up ready to go to the laundry; this chance shape that it now has, in some way or other does not affect 'her cloth', when she is asked to identify it; in a certain sense the shape it has is part of the cloth's 'surroundings', or perhaps we should say its 'surrounding field'.

We can now actually carry out experiments within the field of perception-psychology in order to ascertain a subject's ability to perceive a thing quite directly, in spite of seeing certain aspects of perception as belonging not to the thing itself, but rather to its 'surrounding field'. There are large individual differences in this respect. It is possible that Berkeley was of the type for whom it is difficult to separate certain aspects of a perception and regard these as belonging to the perceived 'something's' surrounding field, while



## BERKELEY AND MODERN PSYCHOLOGY

the perceived 'something' nevertheless is experienced quite directly and with a certain quality of wholeness. For persons who have such difficulties, or wherever it is difficult or impossible to perceive something directly when certain of its aspects are to be regarded as belonging to its surrounding field, we could conventionally introduce the terminology that they all the same 'perceive' this something, but more indirectly and presuming that a number of other things are present that are directly perceived. If we accept this use of words, the statement could be formulated in its general form, taking account of Berkeley's denial of 'abstract ideas': it is impossible to 'perceive' (directly or indirectly) an idea A without at the same time directly perceiving a more closely defined field of ideas of which some belong to A's surrounding field and some to the field of A itself. In this formulation, Berkeley would have been able to use his denial of 'abstract ideas' in his ensuing discussion with just as great effect as he used his original formulation. His main demand is that the statement that something exists must be able to be tested within the directly experienced world. He has used the word 'experience' or 'perceive' in a somewhat more restricted sense than modern psychologists or phenomenologists are inclined to do, but his main thesis is not affected in any way by this.

In his work, *A New Theory of Vision*, Berkeley examines our visual perception of space. We can understand that it was imperative for him to show its subjective character, because space is one of those ideas that can contribute in the highest degree to our impression of something existing independently of ourselves. He attempts to show its subjective nature by demonstrating that visual space arises from a number of elementary visual data being brought to point to purely tactile data by experience and training. Even though Berkeley in this work must be said to base his views on the mosaic psychological standpoint which is not accepted today, many of his discussions and descriptions are still of surprisingly great interest because he does not rigidly adhere to this basic attitude. Sometimes he compares visual space and the way in which its elements point towards tactile elements, to a well-mastered language where the linguistic elements point directly towards the meaning they express, which appears as though it were given 'in and with' the elements. Concerning this phenomenon in space-perception he says quite rightly that it is so direct that it is impossible, or nearly so, to experience the visually sensed elements in isolation from the corresponding tactile elements; but in so doing he leaves

—without realising it—his original basis which, being mosaic psychology, must entail that the visual elements are experienced, that the tactile elements are experienced, and that at the same time a connection is made between them. He thus approaches a more modern phenomenological viewpoint, where we would preferably say that we experience spacial entities as wholes that cannot be reduced to mere combinations of sensory elements, but which can occur with visual and tactile aspects. That these wholes develop with experience and learning just like language, as is postulated by Berkeley, I think no modern psychologist would contradict.

Berkeley's criticism of contemporary optics is exemplary, as is his discussion of the reversed images on the retina, which nevertheless result in our seeing things the right way up. This may be compared with modern investigations, for example Stratton's experiment,<sup>1</sup> where the subjects were equipped with spectacles that reversed the image on the retina. At first everything looked upside down, but after some time the perceptions changed character in a definite way and finally things were seen as upright once again. These experiments have been repeated in an excellent way in Innsbruck by Erismann who, with the help of prisms and mirrors, reversed either only the horizontal relations or only the vertical relations of the stimuli on the retina.<sup>2</sup> He found similar interesting results that supported the assumption of the important role of learning and experience in the development of our visual space, even if we in present-day terminology would not say that the training resulted only in learned connections between previously existing elements, but would preferably say that learning gives rise to new entities which can have some of the previously existing elements as aspects.

Berkeley's statement that visually perceived number is also of a subjective nature was very bold for his time. Here, as in many other cases, he is far ahead of his age. It is curious to read what he says and compare it with the recent demonstration of how the perception of number develops in children, for whom, for instance, acoustically perceived number (two, three or more knocks) at a certain stage in their development has nothing to do with visually perceived number (two, three or more seen objects). We may compare also Ponzo's investigations of perceived quantity, and remember how little per-

<sup>1</sup> G. M. Stratton, 'Vision without inversion of the retinal image', *Psychol. Rev.*, 1897, 4, 341-360, 463-481

<sup>2</sup> Erismann has not yet published his results.



## BERKELEY AND MODERN PSYCHOLOGY

ception of quantity and number in peripheral vision has to do with perception of quantity and number in foveal vision.

We can safely say that by fully accepting Berkeley's postulate about the subjective character of space and his assertion of the meaninglessness of thinking of the perceived entity as in a way duplicated so that we have two identical examples of it—one existing in consciousness and a corresponding one existing independently of any consciousness (an abstract idea in Berkeley's sense)—that only then has the psychology of space-perception made real progress. If space allowed I could give innumerable examples of how the remains of such a naïve realistic 'duplicate conception' has inhibited the development of the psychology of space, and this is also true of many other fields of perception psychology.

If we give the word 'perceive' a more general meaning, so that it also includes our conscious occupation with things belonging to a higher stage of knowledge, as, for instance, physical things which cannot be perceived so directly as everyday things, but which nevertheless are presented to us in a phenomenologically describable way, then in my opinion we could, with just as much right as Berkeley had in discussing directly perceived entities, affirm how meaningless it is to think about the 'perceived' things as duplicated so that we have, in a way, two identical examples, one existing in consciousness, and a corresponding one existing independently of any consciousness (an abstract idea true to the spirit of Berkeley).

No matter what our epistemological leanings, as practical psychologists we must accept these final consequences of Berkeley's statements if we are to investigate the psychology of knowledge empirically and in general, just as we have hitherto investigated the psychology of perception in an empirical way.

Many urgent problems and tasks are posed for us as psychologists as a consequence of Berkeley's way of speaking and thinking, tasks that as yet have not been taken up to any great extent, as, for instance, to give a phenomenological analysis of experiences of 'existence', 'reality' or 'objectivity' within the everyday world, and examine the conditions for their manifestation, just as we examine the conditions for experiences of 'red' or 'triangularity'. Part of this task is to carry out exactly the same investigation with regard to that 'existence', 'reality', or 'objectivity' we work with on higher stages of knowledge, for instance the physical. We must compare the different stages of knowledge in regard to this point. Another

task is to give a phenomenological analysis of the experiences of 'identity' within everyday life, that is to say, of our experiences when we experience that one thing 'is the same as' another, and to extend the investigations in a similar way as in the above mentioned investigation of 'existence'.

Could such problems even be formulated if Berkeley had not dug so deeply into the problem of existence and others had not followed him?

Concerning the epistemological side of Berkeley's work: Is there any more profound or exact way for us human beings to converse about something, and make ourselves understood concerning this something, than to give a careful account of what we each *experience* when we experience this 'something'?—in other words, as carefully as possible to regard everything as existing in consciousness? Our minds are isolated so that the problem arises as to whether we experience 'the same'. But has it any meaning to discuss this without examining what we experience when we experience that one thing is 'the same as' another thing, and without honestly stating at which stage of knowledge we are? Is exactly the same not also true in the discussion of 'existence'? If someone discusses identity and existence independently of what we mean by these words, that is, independently of what we experience, has he not left the community of minds? Was it not this Berkeley actually fought against when he attacked 'abstract ideas', demanding that every meaningful thing we speak about must be capable of being tested within the directly *experienced* world. This demand is the only restriction such points of view as Berkeley's entail regarding the world we live in and try to investigate. On the other hand, they give rise to an enormous extension of the concept of consciousness (namely an infinite series of successive extensions) that is very promising for psychological research. When I see my table before me, and *see* that it is independent and out *there* in space independent of me, my concept of consciousness is narrower than that more extended concept I have when I am taught that this 'perceived table being out there in space independently of me' is actually dependent on what we may come to regard as processes in my central nervous system, just as perceived red is dependent on processes in my central nervous system.

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## PHYSIOLOGICAL MECHANISMS IN THE PERCEPTION OF DISTANCE BY SIGHT AND BERKELEY'S THEORY OF VISION

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IN the history of science, Berkeley's *Essay towards a New Theory of Vision* forms one of the links between Descartes and Helmholtz. For instance, the empiristic theory of Helmholtz<sup>1</sup> is related to Berkeley's dictum that our visual perception of space is entirely derived from experience. Although Berkeley was not primarily a scientist and made but few experiments, his *Essay* reveals a thorough grasp of scientific matters on the empirical plane. Some of the problems he studied have remained the subject of research and discussion by scientists ever since. This is true for instance of Molyneux's problem concerning the man born blind who would recover sight, and of the ever-recurring puzzle of the apparent size of the moon, of which Berkeley gives an 'empiristic' explanation in terms which reveal an obvious feeling for the wonder of natural phenomena. It would hardly be possible to give an up-to-date analysis of the *Essay* without going deeply into scientific matters of great complexity or difficulty. Indeed a full scientific commentary on the *Essay* might have to assume proportions similar to those of Helmholtz's *Physiological Optics*. It would have to sift material scattered in publications pertaining to psychology, physiology and ophthalmology, much of which has accumulated since Helmholtz's time. Even Helmholtz's *Optics* would have to be re-examined critically in order to bring hidden assumptions to the surface. Furthermore, it is most likely that new experimental work would be found to be necessary for a proper understanding of certain subjects such as binocular vision.

I propose here only to discuss certain recent developments in physiological optics which are related to Berkeley's theory of the perception of distance by sight, and then to examine briefly how the *Essay* influenced later scientists.

<sup>1</sup> H. von Helmholtz, *Handbuch der Physiologischen Optik*, Hamburg and Leipzig, 1867

I *The Angles of the Rays Entering the Eye*

Berkeley rejected the theory according to which we compute the distance and position of objects on the basis of the angles of the light rays which originate from the objects and which strike our eyes. Only monocular vision will be examined here. What gives us information about the distance of an object when we use only one eye is, according to Berkeley, the confused (or distinct) appearance of the object, or the 'straining of the eye', that is, the sensation accompanying the effort of accommodation required to see the object distinctly.

To simplify the argument, we will confine our attention to the first of these two factors. Berkeley refers to the confusion perceived by the mind but there is no doubt that in the *Essay* he considers this to be correlated to the confusedness of the retinal image. We will here take this correlation for granted and mostly confine the discussion to the retinal image.

If the rays come from a point of an object very close to the eye, they diverge to such an extent that the (normal or hypermetropic) eye cannot bring them to an accurate focus on the retina. That is, even if the optical system of the eye is strong enough to render these rays convergent, they will converge towards a point situated, not on the retina, but behind it. The rays thus constitute a cone which intersects the retina, forming upon it a 'circle of confusion'. If the rays come from a sufficiently distant source, they diverge less as they enter the eye and accordingly they can be refracted into a cone the apex of which falls on the retina, the image thus being brought into focus. What is of immediate importance in vision, Berkeley said, is the distinctness or the confusion of appearance. This is related to the degree of sharpness or confusedness of the images formed on the retina, which in turn does depend on the angles of the rays, but these angles themselves are not perceived as such. Those unskilled in optics know nothing about the angles of the rays entering their eyes.

Many today would probably feel that Berkeley's theory, at least with regard to the particular case examined here, is a rather obvious one, and they might find it strange that in Berkeley's time it was thought that we could directly know the direction, *qua* direction, of a ray of light impinging upon our retina. Considered as a whole, the problem is a complex one which raises both scientific and methodological difficulties. It is not rare to find, even in contemporary writings on optics, a reference to the 'projection' of the retinal image



## PHYSIOLOGICAL MECHANISMS

into external space. Such a statement, if it is not a mere figure of speech, would appear to imply acceptance of the theory condemned by Berkeley.

It must be borne in mind here that the accommodation mechanism of the eye, in particular the distinction between presbyopia and hypermetropia, was not well understood until the nineteenth century. The formation of the retinal image has been understood since the seventeenth century, but even today the dioptrics of the eye remains a field full of surprises and difficulties. At this point the reader might be warned that a full discussion even of the present limited problem would scarcely be possible within the scope of this paper.

Whereas in the normal eye it is objects very close to the eye which give confused retinal images, in the case of the myopic eye such images are produced by distant objects, near objects being seen clearly. The eyeball of the myopic eye is too long for its optical system. Even when accommodation is relaxed, a sharp image of distant objects is formed in front of the retina, in the vitreous humour, and the light reaching the retina therefore gives an image which is out of focus. This explains that the myope never sees distant objects clearly. Thus in the strong myope, or 'purblind', confusion should be associated with distance, whereas in the normal and hyperope it should be associated with nearness. Berkeley thought that if an artificial arrangement were used to throw a blurred image of an object on to the retinae of a myope and of a hyperope, the former should accordingly think that the object is far away, the latter should think that it is near. Berkeley does not seem to have carried out actual experiments to test this suggestion.

In the myope the rays from a distant point cross at a point in front of the retina before striking it, whereas in the hyperope the rays from a near point giving a confusion circle of similar size do not cross. The angles of incidence of the rays upon the retinae are different in the two cases. Now the basis of the rather complicated experiment suggested by Berkeley is that such differences in the vergence of the light should be undetectable by the eye. 'The eye, or (to speak truly), the mind perceiving only the confusion itself, without ever considering the cause from which it proceeds, doth constantly annex the same degree of distance to the same degree of confusion. Whether that confusion be occasioned by converging or diverging rays, it matters not.'<sup>1</sup> In fact, however, experiments on

<sup>1</sup> *Essay on Vision*, Sect. 36

the accommodation reflex show that at least in certain cases the human eye can detect differences in the vergence of the light striking the retina, other experimental factors being kept constant.

## 2 The Accommodation Reflex

Consider a distant point source of light seen clearly by a normal eye. The light rays reaching the eye are only slightly divergent. Now place a 1- or 2-dioptre negative lens in front of the eye. This blurs the retinal image, at least momentarily. On emerging from this glass lens the rays have become markedly divergent. The optical system of the eye, in which we suppose that no change of accommodation has yet taken place, is not strong enough to bring these diverging rays to focus. The rays form a 'confusion circle' on the retina. An adjustment of accommodation is therefore necessary to restore distinct vision. In young subjects this takes place by reflex action. The eye at once increases the curvature of its own lens, which makes the rays again converge on to one retinal point. This reflex adjustment occurs without hesitation in the right direction. It will be noted that the blurred image before reaccommodating was a confusion circle made by *converging* rays.

Rapidly remove the negative lens after the eye has become adjusted to it. The power of the eye is now too great for the nearly-parallel rays reaching it and the rays converge to a point in front of the retina, whence they diverge to form a confusion circle on the retina. The size of this circle is similar to that of the above, but the circle is formed by *diverging* rays. The point of this experiment is that a reflex adjustment of accommodation once more takes place without hesitation. It takes place in the correct direction, producing a decrease, instead of an increase, in the curvature of the eye lens. This phenomenon, which has recently been studied by Fincham,<sup>1</sup> proves that the reaction of the organism does not depend only on the size of the confusion circle. The visual system does give different responses depending on the state of the light on the retina, whether convergent or divergent.

Now if the light is white, the confusion circles are coloured on account of the chromatic aberration of the eye. The colours are not arranged in the same way in the cases of converging and of diverging rays. In the former case the outer edge of the circle will tend to be

<sup>1</sup> E. F. Fincham, 'The accommodation reflex and its stimulus', *Br. J. Ophthalm.*, 1951, 35, 381-393



## PHYSIOLOGICAL MECHANISMS

red, in the latter it will tend to be blue. Thus chromatic aberration may be the determining factor in the accommodation reflex. This theory finds support in the fact that the reflex fails in many subjects when monochromatic light, such as sodium light, is substituted for white light,<sup>1</sup> whereas it still takes place when a yellow light made up of a mixture of green and red light is used.<sup>2</sup>

These experiments, at least in a narrow sense, seem to contradict Berkeley's contention that it is only 'the confusion itself' which matters. If his attention had been directed to this, Berkeley might have suggested that while the degree of confusion perceived is the same for converging and diverging rays, the sensations may yet differ in some other respect, as in fact they do on account of the chromatic aberration. Alternatively, since we are dealing with an unconscious reflex, it might be argued that the present example cannot affect Berkeley's reasoning which refers to conscious sensation and perception. Yet this does not exhaust the problem. Among other things, the fact that the accommodation reflex is a reflex is disturbing for the empiristic theory, for it tends to narrow the gap between distance known by sight and distance known by touch.

### 3 *The Directional Sensitivity of the Retina*

In certain subjects the accommodation reflex persists in monochromatic light, at least when the subject allows his eye to move while making the adjustment.<sup>3</sup> In such cases it is thought that the 'directional sensitivity' of the retina discovered by Stiles and Crawford<sup>4</sup> provides the basis for the accommodation reflex. The cones of the retina respond more strongly to light falling upon them in the direction of their axis than at an angle to their axis. Suppose a fine pencil of light enters the eye through the centre of the pupil and forms an image on the fovea. This image will appear brighter to the subject of the experiment than another image due to another pencil exactly similar but entering near the edge instead of at the centre of the pupil.

<sup>1</sup> E. F. Fincham, 'The accommodation reflex and its stimulus', *Br. J. Ophthalmol.*, 1951, **35**, 381-393

<sup>2</sup> E. F. Fincham, personal communication; to be published in *J. Physiol.*

<sup>3</sup> E. F. Fincham, *op. cit.*

<sup>4</sup> W. S. Stiles and B. H. Crawford, *Proc. Roy. Soc. (B)*, 1933, **112**, 428-450; *Nature*, 1937, **139**, 246; W. S. Stiles, *Science Progress*, 1939, **33**, 676-689, *Proc. Roy. Soc. (B)*, 1939, **127**, 64-103; Flamant and Stiles, *J. Physiol.*, 1948, **107**, 187-202.

The only difference is that this second pencil falls at a slight angle on to the fovea while the first strikes it normally.

The Stiles-Crawford effect (the theory of which is not yet well understood) shows that changes in angle of the rays can lead to changes in strength of stimulation in the nerve fibres connected to the photo-receptors. By suitably adjusting the physical intensities of the two pencils, making the second up to about 3 or 4 times as intense as the first, the two patches of light can be made to appear of equal intensity. The retinal cones therefore fail to respond to direction *qua* direction. Accordingly this intriguing phenomenon could hardly be taken as a refutation of Berkeley's essential thesis ; yet it does raise difficulties.

#### 4 *Patterns of Stimulation*

From a purely physiological or neurological standpoint, we might say that what determines the reactions of the organism is the retinal pattern of stimulation caused by the incident light, not the distribution of the light itself either inside or outside the eye. In general, the pattern of stimulation of receptors other than those of the retina, for instance of the proprioceptors in the intrinsic and extrinsic eye muscles, is also relevant and must be taken into consideration.

If different external stimuli produce the same peripheral pattern of excitation, the organism will not be able to differentiate between them either by reflex action or by conscious response. On the other hand, if, as in the above experiments, the organism gives a consistently different reaction to two different stimuli, then the peripheral pattern of excitation must be different in the two cases.

Such considerations do not belong to Berkeley's philosophy. Yet in practice, on the empirical plane of scientific investigation, they bear a resemblance to Berkeley's statement quoted at the end of Section 1, at least when such a statement is taken in isolation from the rest of Berkeley's thought.

It seems that the above neurological considerations can be extended to the whole of the nervous system, for instance in the form of the following principle : ' Stimuli which are physically different, but which produce the same reaction at any given level of the nervous system, cannot determine at subsequent levels reactions showing differences which are consistently related to the differences between the initial stimuli.'

Now this principle, implicit in many investigations relating to



## PHYSIOLOGICAL MECHANISMS

sensory physiology,<sup>1</sup> is of course not to be found in the *Essay*. Such a materialistic, mechanistic approach was foreign to Berkeley's philosophy. When dealing with scientific problems as in the passage quoted above, Berkeley may appear to jump from peripheral nervous stimulation to sensations in the mind. This is done by many every day, at least as a manner of speech. But Berkeley never used the purely physiological or neurological method of approach, which takes space and matter for granted and which uses a language designed to avoid any reference to the mind, ideas or sensations.

Yet precisely because such a method of approach is foreign to Berkeley's philosophy, and also because hitherto it has not been extensively used, it may be advantageous to adopt it in scientific studies of the problems relevant to Berkeley's inquiries. The empirical meaning of such studies is too often obscured by the fact that they make use of unformulated philosophical assumptions, usually related to Cartesian dualism. It may even be advantageous to deal systematically with each of these scientific problems from the standpoint of more than one methodological postulate, for instance the purely materialistic and the Cartesian. This may help one to isolate their purely empirical significance.

### 5 *The Essay on Vision and Cartesian Dualism*

There seems to be an element of incompleteness in Berkeley's *Essay on Vision*, and it is perhaps this lack of logical perfection which explains the influence exerted by the *Essay* on Helmholtz and other scientists. In the *Essay*, Berkeley states that our *visual* perception of space derives entirely from experience, but takes it for granted that we know distance by the sense of touch. He proceeds to reduce everything to the latter sense. As Lindsay points out,<sup>2</sup> Berkeley here stops short of the full implications of his doctrine. 'For the objections which he brings forward to visual apply equally well to tangible space . . . if the fundamental ideas of perception are isolated sensible ideas, the perception of space cannot be explained at all.'

Hume developed arguments of this kind into his sceptical philosophy, and Kant attempted to rebuild a philosophy upon the ruins Hume left behind him. The modern reader of the *Essay*, however,

<sup>1</sup> M. H. Pirenne, *Vision and the Eye*, London, 1948, p. xviii

<sup>2</sup> A. D. Lindsay, Introduction to Everyman's edition of Berkeley, *A New Theory of Vision and Other Writings*, London, 1938, p. xviii

may be struck by the fact that Berkeley discusses empirical problems in a language which bears a strong resemblance to that used today by many scientists, and which originated with Descartes. In such passages Berkeley seems to place man in an external world, the extension of which he can know by touch. Light stimuli excite his retina, leading to sensation and perception in his mind. Essentially this is the familiar, often called 'scientific' but in fact Cartesian, conception of the world. It implies the existence of the unexplained, intractable link between matter and mind. Whereas it may be claimed that Berkeley never really meant to use such a conception, yet it is a fact that many readers more interested in science than in philosophy will, rightly or wrongly, read this into his writings.

Now Descartes himself had said that our sensations are little else than symbols of the material objects of the external world acting upon our senses. This view, defended by Helmholtz,<sup>1</sup> is still held by some contemporary scientists and philosophers.<sup>2</sup> It means that there is nothing in common between the nature of our sensations and the nature of the material world. It would appear therefore that the matter of the scientists is in fact unknowable. This leads those who reason like Descartes into a difficulty, for in the argument purporting to prove that matter is unknowable, they speak of material objects as if they knew them.<sup>3</sup> Berkeley asserted that this unknowable Cartesian matter does not exist. Only sensations and ideas in the mind exist. This, of course, eliminates the difficulty of the relationship between mind and matter.

<sup>1</sup> See e.g. M. H. Pirenne, 'On physiology and consciousness', *Brit. J. Psychol.*, 1947, 37, 82-86; 'Descartes and the body-mind problem in physiology', this *Journal*, 1950, 1, 43-59

<sup>2</sup> e.g. by H. Piéron, *The Sensations, their Functions, Processes and Mechanisms*, London, 1952, p. 415

<sup>3</sup> At the end of a paper putting forward the same views as Descartes and Helmholtz, Léon Fredericq ('Nos Sensations et le Monde Extérieur', *Scientia*, 1927, 41, 193-202) pointed out this embarrassing circularity of argument: 'On me fera peut-être une objection. Vous avez cherché à établir, me dira-t-on, qu'il n'y a aucun rapport entre la nature de nos sensations et celle du monde extérieur et que l'idée que le bon sens vulgaire se fait de ce monde qu'il enferme dans un espace à trois dimensions, ne saurait correspondre à la réalité. Pourtant toute votre argumentation est basée sur la croyance à l'existence réelle de ce monde conventionnel, puisque vous parlez d'organes des sens, de fibres et de cellules nerveuses, de lumière, de son, comme si ces choses avaient une existence objective. Il y a là un véritable cercle vicieux. J'avoue que je serais bien embarrassé de trouver une réponse satisfaisante à cette objection.'



## PHYSIOLOGICAL MECHANISMS

In spite of the fact that Berkeley's thought might from a certain standpoint be considered as a *reductio ad absurdum* of Cartesian dualism, the *Essay on Vision* did provide a basis for the development of the physiology and psychology of vision precisely along crypto-Cartesian lines. This no doubt was due to the intrinsic scientific interest of the problems discussed by Berkeley. Many scientists became attracted to these problems while caring little for their philosophical background and implications.

Whereas Berkeley saw the need of a clear understanding of such problems as a basis for philosophical discussion, this has perhaps not been so fully realised by all the philosophers who followed him. This may explain why some contemporary philosophical theories may give rise to considerable uneasiness in the minds of scientific readers, who will nevertheless feel at once in sympathy with much of what Berkeley said in his *New Theory of Vision*. The deep modern cleavage between science and philosophy seems detrimental to both fields of inquiry.<sup>1</sup>

<sup>1</sup> I am greatly indebted to Professor E. F. O'Doherty for discussions on the above subject.

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# BERKELEY'S CRITICISM OF THE INFINITESIMAL

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In his two notebooks, now known as *Philosophical Commentaries*, which contain the raw materials of his thought when he was a very young man, Berkeley made a number of critical remarks about mathematics. It was more than twenty-five years, however, before he developed these and published his tract *The Analyst* (1734). Historians of mathematics have recognised its value, but philosophers have generally supposed that he was indulging in a futile trial of strength with Newton on Newton's home ground.

Berkeley attacked the logic of the method of fluxions or infinitesimal calculus, holding that the infinitesimal was a zero-increment, a finite quantity of no size, that it was treated at one stage as finite and at another as zero as convenience dictated, that its effects were retained after it was made to vanish—that in fact it was self-contradictory. His two ways of bringing this out are the acme of lucidity; one concerns the fluxion of a power, the other that of a product.

He deals with the fluxion of  $x^n$ , using the binomial expansion. For brevity I will make his point by considering  $x^2$ . When  $x$  'flows', as he puts it, he calls the increment  $o$ . The incrementary ratio is  $(2x \cdot o + o^2)/o$  or  $2x + o$ . He notes that  $o$  is here supposed to be 'something'. The next step, however, is to let  $o$  become zero, so as to produce the fluxion  $2x$ . Of this Berkeley says there is now introduced a supposition contrary to the first, namely, that there is now no increment of  $x$  (or that  $o$  is now nothing), so that it is invalid to retain the result  $2x$ , because this was arrived at by supposing the  $o$  was something. In short, if  $o$  is something what is obtained is not  $2x$  by  $2x + o$  where  $o$  is not zero; while, if  $o$  equals zero, nothing at all is obtained.

Thus Berkeley dismissed fluxions as 'ghosts of departed quantities'. This was a major achievement of logical criticism, even though he did not recognise that Newton's intuition was pointing in a most significant direction or that a satisfactory theory of limits might be possible. Still, he went to the heart of the flaw in the Newtonian (and Leibnizian) presentation.

The error involved has a living interest, seeing that nearly all



## BERKELEY'S CRITICISM OF THE INFINITESIMAL

elementary textbooks on the differential calculus written in English develop the first steps incorrectly. A certain amount of mathematical analysis is required for cases where it looks permissible to put the increment equal to zero.

With regard to Newton's method of obtaining the fluxion of product, Berkeley showed that with a little ingenuity any number of different results could be obtained. As late as 1862 the great mathematician, Sir William Rowan Hamilton, wrote to De Morgan suggesting that Berkeley was right and that Newton's way of getting the right answer involved 'artifice' and was 'sophistical', 'although I should not like to say so publicly'.

The ensuing controversy in which Berkeley joined was violent and prolonged. Mathematicians were ranged on both sides, Maclaurin being on Berkeley's. The problem was resolved, so far as Berkeley's criticism was concerned, in 1821 by Cauchy's theory of limits (though further refinements have continued to follow). The outcome is that Berkeley has been proved correct in his criticism: the concept of the infinitesimal had to be eliminated from the theory. At the same time it became clear that Newton's theory, with suitable amendments and additions, could be soundly based.

It seems to me that in the field of contributions of well-nigh the highest rank, Berkeley's criticism, despite its being purely destructive, is one of the most important.

Berkeley went on to enquire how the method of fluxions, assumed to be faulty, could produce correct results in geometry. To explain this he produced an ingenious thesis that there is a compensation of errors, that is, the *one* error introduced into the incrementary ratio is compensated by *one* error in the expression of geometrical properties in terms of infinitesimals. He gives well-constructed examples to support this, and interestingly enough his interpretation was accepted by such eminent mathematicians as Lagrange and Lazare Carnot.

Berkeley and Carnot showed that the calculation of the subtangent, for instance, can be set out in such a way as to contain two finite quantities that cancel each other. Newton treated these quantities as infinitesimals, that is, he made them vanish. According to Berkeley's fundamental contribution this was illegitimate. Hence Newton's method was one in which two cancelling quantities were not cancelled but ignored. The explanation of the correct results given by Berkeley and Carnot was thus that the ignored quantities

## J. O. WISDOM

if restored would cancel. This is correct ; but it is not the only explanation. An alternative, which would seem to be more fundamental, is that Newton's correct results can be obtained with a rigorous theory of limits.

How are we to interpret Newton's own procedure? It is not true that he was attempting to work with finite quantities alone or that he realised these quantities would cancel. Thus the explanation of correct results as due to compensating errors does not constitute an interpretation of what Newton was trying to do (nor did Berkeley suppose that it was). Newton ignored the suspect quantities ; nonetheless, though this rightly evoked Berkeley's criticism, he ignored them, it would seem, because he was working with an intuitive conception of a limit which seemed to justify his equating them to zero. Thus Newton's method, as he appears to have conceived it, would be justified not by its being a telescoped version of one in which errors cancel but by its being a confused version of one in which a precisely defined limit is attained.

That Lagrange and Carnot were satisfied with the interpretation of the method of fluxions as based on a compensation of errors shows how unsure in their understanding of the differential calculus mathematicians were at the end of the eighteenth century.

Berkeley was but little creative as a mathematician, and he seems to have had limited power of thinking in general mathematical terms. On the other hand, this is a merit in a critic, and he certainly gave illustrations of his contentions. Though schoolboys read the differential calculus today, we must remember, in assessing Berkeley's work, that they are presented with a theory that is not fully intelligible, as Berkeley showed. In this field he was in the best sense an amateur, but not amateurish. In the history of mathematics he finds a respected place, and he is allotted this by both Cantor and Cajori in their histories of the subject.

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## BERKELEY'S CRITICISM OF THE INFINITESIMAL

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# A NOTE ON BERKELEY AS PRECURSOR OF MACH

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## I

THE purpose of this note is to give a list of those ideas of Berkeley's in the field of the philosophy of physics which have a strikingly new look. They are mainly ideas which were rediscovered and re-introduced into the discussion of modern physics by Ernst Mach and Heinrich Hertz, and by a number of philosophers and physicists, some of them influenced by Mach, such as Bertrand Russell, Philip Frank, Richard von Mises, Moritz Schlick,<sup>1</sup> Werner Heisenberg, and others.

I may, perhaps, say at once that I do not agree with most of these positivistic views. I admire Berkeley without agreeing with him. But criticism of Berkeley is not the purpose of this note, and will be confined to some very brief and incomplete remarks in section 5.

Berkeley wrote only one work, *De Motu*, devoted exclusively to the philosophy of physical science, but there are passages in many of his other works<sup>2</sup> in which similar ideas, and supplementary ones, are represented.

The core of Berkeley's ideas on the philosophy of science is in his criticism of Newton's dynamics. (Newton's mathematics were criticised by Berkeley in *The Analyst* and its two sequels.) Berkeley had a great admiration for Newton, and no doubt realised that there could have been no worthier object for his criticism.

## 2

The following twenty-one theses are not always expressed in Berkeley's terminology; their order is not connected with the order

<sup>1</sup> Schlick, under the influence of Wittgenstein, suggested an instrumentalist interpretation of universal laws practically equivalent to Berkeley's 'mathematical hypotheses'.

<sup>2</sup> I shall refer, apart from DM (= *De Motu*, 1721), to TV (= *Essay towards a New Theory of Vision*, 1709); Pr (= *Treatise concerning the Principles of Human Knowledge*, 1710); HP (= *Three Dialogues between Hylas and Philonous*, 1713); Alc (= *Alciphron*, 1732) and S (= *Siris*, 1744).



## BERKELEY AS PRECURSOR OF MACH

in which they appear in Berkeley's writings, or in which they might be presented in a systematic treatment of Berkeley's thought.

For a motto, I open my list by a quotation from Berkeley (*DM* 29).

(1) 'To utter a word and mean nothing by it is unworthy of a philosopher.'

(2) The words 'absolute space' and 'absolute time' are without any empirical (or operational) meaning; Newton's doctrine of absolute space and absolute time must therefore be discarded as a physical theory. (Cf. *Pr* 97, 99, 116; *DM* 53, 55, 62; *S* 271: 'Concerning absolute space, that phantom of the mechanic and geometrical philosophers, it may suffice to observe that it is neither perceived by our sense, nor proved by our reason. . . .'; *DM* 64: 'for . . . the purpose of the philosophers of mechanics . . . it suffices to replace their "absolute space" by a relative space determined by the heavens of the fixed stars. . . . Motion and rest defined by this relative space can be conveniently used instead of the absolutes. . . .')

(3) The same holds for the word 'absolute motion'. The principle that all motion is relative can be established by considering the meaning of 'motion', and also upon operationalist arguments. (Cf. *Pr* as above, 58, 115: 'To denominate a body "moved" it is requisite . . . that it changes its distance or situation with regard to some other body . . .'; *DM* 63: 'No motion can be discerned or measured, except with the help of sensible things'; *DM* 62: ' . . . the motion of a stone in a sling or of water in a whirled bucket cannot be called truly circular motion . . . by those who define [motion] with the help of absolute space. . . .')

(4) The word 'force' is misused in physics; to introduce force as the cause or 'principle' of motion (or of an acceleration) is to introduce an occult quality. (*DM* 1-4, and esp. 5, 10, 11, 17, 22, 28; *Alc* vii, 9; *DM* 6: 'It is plain, then, that it is useless to proffer gravity or force as the principle of motion; for how can this principle be any better known by appealing to what is [commonly] called an occult quality? That which is itself occult explains nothing. It goes without saying that an unknown acting cause is more properly called a [metaphysical] substance rather than a quality.')

(5) In view of these considerations, Newton's theory cannot be accepted as an explanation which is truly *causal*, i.e. based on natural causes. The view that gravity causally explains the motion of bodies

(that of the planets, of free-falling bodies, etc.), or that Newton discovered that gravity or attraction is 'an essential quality' (*Pr* 106) whose inherence in the essence or nature of bodies explains the laws of their motion, must be discarded (*S* 234; see also *S* 246, last sentence). *But it must be admitted that Newton's theory leads to the correct results* (*DM* 39, 41). To understand this, 'it is of the greatest importance . . . to distinguish between *mathematical hypotheses* and the *natures* [or *essences*] of things . . . If we observe this distinction, then all the famous theorems of mechanical philosophy which . . . make it possible to subject the world system [i.e. the solar system] to human calculations, may be preserved; and at the same time, the study of motion will be freed from a thousand pointless trivialities and subtleties, and from [meaningless] abstract ideas' (*DM* 66).

(6) In physics (mechanical philosophy), there is no causal explanation (cf. *S* 231), i.e. no explanation based upon the discovery of the hidden nature or essence of things (*Pr* 25). ' . . . real efficient causes of the motion . . . of bodies do not in any way belong to the field of mechanics or of experimental science. Nor can they throw any light on these . . . ' (*DM* 41).

(7) The reason is, simply, that physical things have no secret or hidden, 'true or real nature', no 'real essence', no 'internal qualities' (*Pr* 101).

(8) There is nothing physical *behind* the physical bodies, no occult physical reality. *Everything is surface*, as it were; physical bodies are nothing but their qualities. *Their appearance is their reality* (*Pr* 87, 88).

(9) The province of the scientist (of the 'mechanical philosopher') is the discovery, 'by experiment and reasoning' (*S* 234), of *Laws of Nature*, that is to say, of the regularities and uniformities of natural phenomena.

(10) The Laws of Nature are, in fact, regularities or similarities or analogies (*Pr* 105) in the perceived motions of physical bodies (*S* 234) ' . . . these we learn from experience ' (*Pr* 30); they are observed, or inferred from observations (*Pr* 30, 62; *S* 228, 264).

(11) 'Once the Laws of Nature have been formed, it becomes the task of the philosopher to show of each phenomenon that it is in conformity with these laws, that is, necessarily follows from these principles.' (*DM* 37; cf. *Pr* 107; and *S* 231: 'their [i.e. the "mechanical philosophers"] province being . . . to account for particular phenomena by reducing them under, and shewing their conformity to, such general rules.')



## BERKELEY AS PRECURSOR OF MACH

(12) This process *may* be called, if we like, 'explanation' (even 'causal explanation'), so long as we distinguish it clearly from the truly causal (i.e. metaphysical) explanation based upon the true nature or essence of things. (S 231; DM 37: 'A thing may be said to be mechanically explained if it is reduced to those most simple and universal principles' (i.e. 'the primary laws of motion which have been proved by experiments . . . ' DM 36) 'and proved, by accurate reasoning, to be in agreement and connection with them. . . This means to *explain* and solve the phenomena, and to assign them their *cause* . . . ' This terminology is admissible (cf. DM 71) but it must not mislead us. We must always clearly distinguish (cf. DM 72) between an 'essentialist'<sup>1</sup> explanation with appeals to the nature of things and a 'descriptive' explanation which appeals to a Law of Nature, i.e. to the description of an observed regularity. Of these two kinds of explanation, only the latter is admissible in physical science.

(13) From both of these, we must now distinguish a third kind of 'explanation'—an explanation which appeals to *mathematical hypotheses*. A mathematical hypothesis may be described as a procedure for calculating certain results. It is a mere formalism, a mathematical tool or instrument, comparable to a calculating machine. It is judged merely by its efficiency. It is not only admissible, but it may be admirably useful. But, as opposed to the explanation by essences (which, in mechanics, are simply false) and to that by laws of nature (which are, if the laws 'have been proved by experiment', simply true), the question of the *truth* of a mathematical hypothesis does not arise—only that of its *usefulness as a calculating tool*.

(14) Now, those principles of the Newtonian theory which 'have been proved by experiment'—those of the laws of motion which simply describe the observable regularities of the motion of bodies—are true. But the part of the theory involving the concepts which have been criticised above—absolute space, absolute motion, force, attraction, gravity—is not true, since these are 'mathematical hypotheses'. As such, they should not, however, be rejected, if they work well (as in the case of force, attraction, gravity). Absolute space and absolute motion have to be rejected because they do not work (they are to be replaced by the system of fixed stars, and

<sup>1</sup> The term 'essentialist' (and 'essentialism') is not Berkeley's but was introduced by me in 'The Poverty of Historicism', *Economica*, N.S. II, 1944, and in my *Open Society and Its Enemies*.

motion relative to it). “Force”, “gravity”, “attraction”,<sup>1</sup> and words such as these are useful for purposes of reasoning and for computations of motions and of moving bodies; but they do not help us to understand the simple nature of motion itself, nor do they serve to designate so many distinct qualities. . . . As far as attraction is concerned it is clear that it was not introduced by Newton as a true physical quality but merely as a mathematical hypothesis’ (*DM* 17).<sup>1</sup> ‘But what is said of forces residing in bodies, whether attracting or repelling, is to be regarded only as a mathematical hypothesis, and not as anything really existing in nature.’ (*S* 234; cf. *DM* 18, 39 and esp. *Alc* vii, 9.)

(15) Properly understood, a mathematical hypothesis does not claim that anything exists in nature which corresponds to it—neither to the words or terms with which it operates, nor to the functional dependencies which it appears to assert. It erects, as it were, a fictitious mathematical world behind that of appearance, but without the claim that this world exists. It claims only that from its assumptions, the correct consequences can be drawn. But it can be easily misinterpreted as claiming more; that is to say, as claiming to describe a real world behind the world of appearance. But such a world *cannot* be described; for such a description would be meaningless.

(16) It can be seen from this that the same appearances *may* be successfully calculated from more than one mathematical hypothesis, and that two mathematical hypotheses which yield the same results concerning the calculated appearances may not only differ, but even contradict each other (especially if they are misinterpreted as describing a world of essences behind the world of appearances); nevertheless, there may be nothing to choose between them. ‘The foremost of men proffer . . . many different doctrines, and even opposite doctrines, and yet their conclusions [i.e. their calculated results] attain the truth . . . Newton and Torricelli seem to disagree with one another, . . . but the thing is well enough explained by both. For all forces attributed to bodies are merely mathematical hypotheses . . .; thus the same thing may be explained in different ways’ (*DM* 67).

(17) The analysis of Newton’s theory thus yields the following results:

<sup>1</sup> The italics in the Latin original appear to function here as quotes.

## BERKELEY AS PRECURSOR OF MACH

We must distinguish

- (a) Observations of concrete, particular things.
- (b) Laws of Nature which are either observations of regularities or (*DM* 36) proved (*comprobatae*) by experiments, or (*Pr* 107) discovered 'by a diligent observation of the phenomena'.
- (c) Mathematical hypotheses which are not based on observation but whose consequences agree with the phenomena (or 'save the phenomena', as the Platonists said).
- (d) Essentialist or metaphysical causal explanations which have no place in physical science.

Of these four, (a) and (b) are based on observation, and can be, from experience, known to be true; (c) is not based on observation and has only an instrumental significance—thus more than one instrument may do the trick; and (d) is known to be false whenever it constructs a world of essences behind the world of appearances. Thus (c) is known to be false whenever it is claimed to have the status of (d).

(18) These results, clearly, apply to cases other than Newtonian theory, for example, to atomism (corpuscular theory). In so far as this theory attempts to explain the world of appearances by constructing an invisible world of 'inward essences' (*Pr* 102) behind the world of appearances, it must be rejected. (Cf. *Pr* 50; *S* 232, 235.)

(19) The work of the scientist leads to something that may be called 'explanation', but it is hardly of great value for *understanding* the thing explained, since the attainable explanation is not one based upon an insight into the nature of things. But it is of practical importance. It enables us to make both *applications* and *predictions*. '... laws of nature or motions direct us how to act, and teach us what to expect' (*S* 234; cf. *Pr* 62). Prediction is based merely upon regular sequence (not upon causal sequence—at least not in the essentialist sense). A sudden darkness at noon may be a 'prognostic' indicator, a warning 'sign', a 'mark' of the coming downpour: nobody takes it as its cause. Now *all* observed regularities are of this nature, even though 'prognostics' or 'signs' are usually mistaken for true causes. (*TV* 147; *Pr* 44, 65, 108; *S* 252-254; *Alc* iv, 14, 15.)

(20) A general practical result of the analysis of physics may be called 'Berkeley's razor'. It allows us *a priori* to eliminate from physical science all essentialist explanations. If they have a mathematical and a predictive content, they may be admitted *qua*



mathematical hypotheses (while their essentialist interpretation is eliminated). If not, they may be ruled out altogether. This razor is sharper than Ockham's: *all* entities are ruled out except those which are perceived.

(21) The ultimate argument for these views, the reason why occult substances and qualities, physical forces, structures of corpuscles, etc., absolute space, and absolute motion, are eliminated, is this: we know that there are no entities such as these because we know that the words professedly designating them must be meaningless. To have a meaning, a word must stand for an 'idea' (or a 'notion'; but words belonging to physical science cannot stand for 'notions'). Now the words here in question do not stand for ideas. 'Those who assert that active force, action, and the principle of motion are really in the bodies, maintain a doctrine that is based upon no experience, and support it by obscure and general terms, and do not themselves understand what they wish to say' (DM 31).

## 3

Everybody who reads this list of twenty-one theses must be struck by its modernity. It is astonishingly similar, especially in its criticism of Newton, to the philosophy of physics which Ernst Mach taught, for many years, in the conviction that it was new and revolutionary; in which he was followed, for example, by Joseph Petzold; and which had an immense influence on modern physics, especially on the Theory of Relativity. There is only one difference: Mach's 'principle of the economy of thought' (*Denkoekonomie*) goes, no doubt, beyond what I have called 'Berkeley's razor' in so far as it allows us not only to discard certain 'metaphysical elements', but also to distinguish in some cases between various competing hypotheses (of the kind called by Berkeley 'mathematical'), with respect to their *simplicity*. Very striking also is the similarity with Hertz's *Principles of Mechanics* (1894), in which he tried to eliminate the concept of 'force', and with Wittgenstein's *Tractatus*.

What is perhaps most striking is that Berkeley and Mach, both great admirers of Newton, criticise the ideas of absolute time, absolute space, and absolute motion, on very similar lines. Mach's criticism, exactly like Berkeley's, culminates in the suggestion that Newton's arguments in favour of his absolute space (Foucault's pendulum, the rotating bucket of water, the effect of centrifugal forces upon the

## BERKELEY AS PRECURSOR OF MACH

shape of the earth) fail because these movements are relative to the system of the fixed stars.

To gauge the significance of this anticipation of Mach's criticism, I shall produce two quotations, one from Mach and one from Einstein. Mach wrote (in the 7th edition of the *Mechanics*, 1912, Ch. 2, sect. 6, § 11) of the reception of his criticism of *absolute motion*, propounded in earlier editions of his *Mechanics*: 'Thirty years ago, the view that the notion of "absolute motion" is meaningless, without any empirical content, and scientifically without use, was generally felt to be very strange. Today, this view is upheld by many and by well-known investigators.' And Einstein said, in his obituary notice for Mach ('Nachruf auf Mach', *Physikalische Zeitschr.*, 1916), with reference to this view of Mach's: 'It is not improbable that Mach would have found the Theory of Relativity if, at a time when his mind was still young, the problem of the constancy of velocity of light had agitated the physicists.' This remark of Einstein's is no doubt more than generous. Of the bright light it throws upon Mach, some reflection must fall upon Berkeley.<sup>1</sup>

### 4

A few words may be said about the relation of Berkeley's philosophy of science to his metaphysics. This is in many respects very different indeed from Mach's.

While the positivist Mach was an enemy of all traditional, that is non-positivistic, metaphysics, and especially of all theology, Berkeley was a Christian theologian, and intensely interested in Christian apologetics. While Mach and Berkeley agreed that such words as 'absolute time', 'absolute space' and 'absolute motion' are meaningless and therefore to be eliminated from science, Mach surely would not have agreed with Berkeley on his reasons why physics cannot treat of real causes. Berkeley believed in causes, even in 'true' or 'real' causes; but all true or real causes were, to him, 'efficient or final causes' (S 231) and therefore *spiritual*, and utterly beyond physics (cf. *HP* ii). He also believed in true or real causal *explanation* (S 231) or, as I may perhaps call it, in 'ultimate explanation'. This, for him, was God.

All appearances are truly caused by God, and explained through

<sup>1</sup> This is not the place to discuss other predecessors of Mach, such as Leibniz.

God's intervention. This, for Berkeley, is the simple reason why physics can only describe regularities, not find true causes.

It would be a mistake, however, to think that the similarity between Berkeley and Mach is, by these differences, shown to be only superficial. On the contrary, Berkeley and Mach are both convinced that there is no physical world (of primary qualities, or of atoms ; cf. *Pr* 50 ; *S* 232, 235) behind the world of physical appearances. Both believed in a form of the doctrine nowadays called phenomenism—the view that physical things are bundles, or complexes, or constructs of phenomenal *qualities*, of particular experienced colours, noises, etc. Mach calls them 'complexes of elements'. The difference is that for Berkeley, these are directly caused by God. For Mach, they are just there. While Berkeley says that there can be nothing physical behind the physical phenomena, Mach suggests that there is nothing at all behind them.

## 5

The great historical importance of Berkeley lies, I believe, in his protest against essentialist explanations in science. Newton himself did not interpret his theory in the sense of essentialism ; he did not believe himself to have found that physical bodies, by their nature, are not only extended but endowed with a force of attraction (radiating from them, and proportionate to the amount of matter in them). But the essentialist interpretation of his theory soon became the ruling one, and remained so up to the days of Mach.

In our own day, essentialism has been dethroned ; a Berkeleian or Machian positivism or instrumentalism has, after all these years, become fashionable.

Yet, clearly, there is a third possibility.

Essentialism is, I believe, untenable. It implies the idea of an *ultimate* explanation, for an essentialist explanation is neither in need of, nor capable of, further explanation. (If it is in the nature of a body to attract others, then there is no need to ask for an explanation of this fact, and no possibility of finding such an explanation.) Yet we know, at least since Einstein, that explanation may be pushed, unexpectedly, further and further.

But although we must reject essentialism, this does not mean that we have to accept positivism.

I shall not here discuss the positivist dogma of meaning, since I



## BERKELEY AS PRECURSOR OF MACH

have done so elsewhere. I shall make only six observations. (a) One can work with something like a world 'behind' the world of appearance without committing oneself to essentialism (especially if one assumes that we can never know whether there is not a further world behind that world). To put it less vaguely, one can work with the idea of hierarchical levels of explanatory hypotheses. There are comparatively low level ones (somewhat like what Berkeley had in mind when he spoke of 'Laws of Nature'); higher ones such as Kepler's laws, still higher ones such as Newton's theory, and, next, Relativity. (b) These theories are not mathematical hypotheses, that is *nothing but* instruments for the prediction of appearances. Their function goes very much further; for (c) there is no pure appearance or pure observation: what Berkeley had in mind when he spoke of these things is always the result of interpretation, and (d) has therefore a theoretical or hypothetical admixture. (e) New theories, moreover, may lead to re-interpretation of old appearances, and in this way change the world of appearances. (f) The multiplicity of explanatory theories which Berkeley noted (see Section 2, § 16, above) is used, wherever possible, to construct, for any two competing theories, conditions in which they yield different observable results, so that we can make a crucial test to decide between them, winning in this way new experience.

A main point of this third view is that science aims at *true* theories, even though we can never be certain, about any particular theory, that it is true; and that science *may* progress (and know that it does so), inventing theories which, compared with earlier ones, may be described as better approximations to what is true.

Such a view can, without becoming essentialist, admit that we always attempt, in science, to explain the known by the unknown, the observed (and observable) by the unobserved (and, perhaps, unobservable). At the same time, it can fully admit, without becoming instrumentalist, what Berkeley said of the nature of hypotheses in the following passage (S 228), which shows the weakness of his analysis (its failure to realise the hypothetical character of what he calls 'laws of nature') and its strength (its admirable understanding of the logical structure of hypothetical explanation):

It is one thing to arrive at general laws of nature from a contemplation of the phenomena; and another to frame an hypothesis, and from thence deduce the phenomena. Those who suppose epicycles, and by them explain the motions and appearances of the planets, may

K. R. POPPER

not therefore be thought to have discovered principles true in fact and nature. And, albeit we may from the premises infer a conclusion, it will not follow that we can argue reciprocally, and from the conclusion infer the premises. For instance, supposing an elastic fluid, whose constituent minute particles are equidistant from each other, and of equal densities and diameters, and recede one from another with a centrifugal force which is inversely as the distance of the centres ; and admitting that from such supposition it must follow that the density and elastic force of such fluid are in the inverse proportion of the space it occupies when compressed by any force ; yet we cannot reciprocally infer that a fluid endued with this property must therefore consist of such supposed equal particles.

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## BERKELEY'S PHILOSOPHY OF MOTION

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It is one of the ironies of history that Berkeley's criticism of Newton's ideas of absolute space and motion was based on arguments which were less theological in character than those which influenced the author of *Principia*, despite the fact that in Berkeley's philosophy the physical universe had no existence except in the sensations which God presents to us and that God alone was responsible for arranging that the appearances of things shall seem to us to conform to what we call the laws of nature. Although Newton himself was no infidel, the Newtonian system was a powerful influence on those who regarded the universe simply as a gigantic machine, and Berkeley was therefore anxious to undermine Newton's authority. Unlike Newton he was by temperament a typical man of the eighteenth century. Consequently, in attacking Newton's ideas he did not revert to traditional modes of thought (theological, Aristotelian or even Cartesian) and therein lies the strength and abiding interest of his attack. Opposed to 'free thinking' as a mask for religious infidelity, he was a brilliant advocate of 'free thinking' in philosophy, in particular in the 'new philosophy' of the mathematical and physical sciences. 'The same impatience of authority in matters of speculation, the same passion for clearness and simplicity, the same dislike of what was either pedantic on the one side or rhetorical on the other, the same desire to clothe his thoughts in an agreeable literary dress, is found in him as in any French philosopher who undertook to acquaint admiring salons with the latest fashion in infidelity. His creed indeed was a different one from theirs, but he belonged to the same century, intellectually as well as chronologically.' So writes one of his biographers.<sup>1</sup>

Newton's conception of the universe was based on his ideas of absolute space and time. He was greatly influenced by the Cambridge Platonists, notably by Henry More. More, in his turn, had been influenced by Descartes who regarded spatial extension as a necessary attribute of existence, but he went beyond Descartes in assigning this

<sup>1</sup> A. J. Balfour, 'Biographical Introduction' to *The Works of George Berkeley*, D.D., London, 1897, vol. I, p. xiv



attribute to God as well as to matter. Writing to Descartes, More declared :

Now the reason which makes me believe that God is extended in this fashion, is that he is omnipresent, and fills intimately the whole universe and each of its parts ; for how could he communicate motion to matter, as he has done betimes, and as he is actually doing according to you, if he did not have immediate contact with matter. . . . God is therefore extended and expanded after his fashion ; whence God is an extended being.

In reply, Descartes argued that God is extended in *power*, so that he can move matter at any point, but this did not mean that he must be geometrically extended. More could not agree, and in opposition to Descartes maintained that if a vase were emptied of air it would not necessarily collapse, because its sides could still be kept apart by the divine extension. In Descartes' view space was identified with material extension. Hobbes, in analysing the doctrine of primary and secondary qualities, had distinguished between space and extension, for he claimed that, whereas you can imagine matter annihilated, you cannot think away space. He drew the conclusion that the idea of space is a mental fiction, but More came to an entirely different conclusion. He argued that, if space cannot be thought away, then it must be the ultimate reality underlying the physical universe and must possess properties which are also attributable to the Deity.

More was also an enthusiastic believer in the infinity of space and in the plurality of worlds, which he regarded as essential consequences of God's infinite nature.<sup>1</sup> Newton not only accepted this belief but even sought further support for it in his theory of universal gravitation. Writing to Bentley in 1692 he argued that unless space were infinite in extent all the matter of the universe would by consequence of its gravity 'fall down into the middle of the whole space and there compose one great spherical mass'. Later, in a famous passage in the *Opticks* he wrote ' . . . does it not appear from Phaenomena that there is a Being incorporeal, living, intelligent, omnipresent who in infinite Space, as it were in his Sensory, sees the things themselves intimately and thoroughly perceives them, and comprehends them wholly by their immediate presence to himself'.<sup>2</sup> Newton maintained that in the case of finite creatures the sense organs are not

<sup>1</sup> A. O. Lovejoy, *The Great Chain of Being*, Harvard, 1942, p. 125

<sup>2</sup> I. Newton, *Opticks*, London, 1931, p. 370

## BERKELEY'S PHILOSOPHY OF MOTION

required for their Sensoria (minds) to perceive 'the Species of Things', but only so that these can be conveyed to their Sensoria. God, he said, has no need of such organs, for he is ubiquitous and his Sensorium is everywhere.

Although Newton's ideas in these matters strike us as more antiquated than those of Berkeley, there is a close similarity between their conceptions of God's relation to physical phenomena. Consequently, Berkeley had every reason to seek out the weak points in Newton's natural philosophy and to make sure that his own was not vulnerable in similar fashion. Newton's conception of the world-process was basically 'metaphysical', but he believed that its actual course can be ascertained only by its effects on observable phenomena. Unfortunately, Newton became entangled in a serious difficulty. He interpreted his famous experiment with a rotating bucket as empirical evidence supporting his metaphysical-theological belief, derived from More, that space is absolute. On the other hand, the successful development of astronomy and dynamics seemed to him to demand what we nowadays call the special relativity principle. According to this principle, all (Euclidean) spaces moving with respect to each other with all conceivable uniform speeds in all conceivable directions have the same physical status. How are all these spaces to be isolated from another similar class, all the members of which are accelerated, and how, assuming that the privileged 'inertial class' has been isolated, are we to determine that fundamental space which he believed to be the ultimate absolute space? Dynamical experiments can assist us in deciding which spaces are to be regarded as inertial, but no such experiments permit us to distinguish one inertial space as more absolute than another. Newton realised that the only possibility lay in astronomical theory and observation.

Despite its dependence on the idea of the relativity of motion, the Copernican system retained the sphere of fixed stars as an absolute system of reference. The final shattering of this outer wall of the medieval universe, notably by Giordano Bruno (himself anything but a scientist!), was a decisive fact for the great minds of the seventeenth century. Even Pascal, who could find no conclusive evidence for adhering to the Copernican system, was haunted by 'les espaces infinis' and maintained that the universe is an infinite sphere of which the centre is everywhere and the circumference nowhere.<sup>1</sup> The idea

<sup>1</sup> B. Pascal, *Pensées* (transl. W. F. Trotter), London, 1931, p. 16

of infinity was a puzzling one. Galileo drew attention to a mathematical paradox associated with it, and the notion of an infinity of centres of the world left men bewildered. Kepler placed the sun at the true centre of the world, being far more thorough-going in this respect than Copernicus, in whose system the centre of the world was the centre of the Earth's orbit and this was not strictly the Sun. Forced to reconsider the problem, what could Newton do? He did not believe that he could use the stars to pick out fundamental centre or privileged space. He was therefore compelled to fall back on the solar system, but he was able to go deeper into the question than Kepler and Copernicus. He placed the centre of the system of the world at the centre of gravity of the solar system. Because of the relative isolation of this system from the other stars, this choice of 'centre' was remarkably successful in practice. Unfortunately, Newton did not excuse his choice by a frank avowal of its pragmatic character, so that his argument is condemned by Weyl as 'a hypothesis unfounded in experience and a dialectical dodge which strikes a discordant note in the midst of the magnificent and cogent inductive development of his system of the world in the third book of the *Principia*'.<sup>1</sup> Moreover, Newton's choice ran directly counter to his own belief in the plurality of worlds. However successful in practice his mathematical physics might prove to be, his philosophy of motion and nature contained an internal contradiction.

It was the great merit of Berkeley's essay, *De Motu*, to expose this fundamental flaw in Newton's system. It appears to have been written at Lyons and was sent to the Paris Academy of Sciences in response to the offer of a prize for an essay on the cause of motion. Berkeley failed to win the prize but published his essay in the original Latin in London in 1721. To publish a criticism of the foundations of Newton's *Principia* in the England of the early eighteenth century must have required great intellectual courage.<sup>2</sup> Translated, the subtitle is 'The principle and nature of motion and the cause of the communication of motions'.<sup>3</sup> It extends only to seventy-two paragraphs, none of great length. Nevertheless, this short essay was in its modest way a major contribution to natural philosophy.

<sup>1</sup> H. Weyl, *Philosophy of Mathematics and Natural Science*, Princeton, 1949, p. 100

<sup>2</sup> A. V. Vasiliev, *Space Time Motion* (transl. H. M. Lucas and C. P. Sayer), London, 1924, p. 63

<sup>3</sup> A. A. Luce, *The Works of George Berkeley, Bishop of Cloyne*, London and Edinburgh, 1951, vol. 4, p. 1



## BERKELEY'S PHILOSOPHY OF MOTION

Berkeley's whole philosophy was based on his rejection of abstract general ideas. In particular, he rejected the ideas of absolute space and time as objective realities existing independently of our perception. Born in 1685, he began to formulate his criticism of Newton's ideas before he was twenty. He devoted several paragraphs of his *Principles of Human Knowledge* (1710) to this question, concluding that the chief advantage of his rejection of the idea of pure space 'exclusive of all body' was that it freed us from the dangerous dilemma of thinking 'either that Real Space is God, or else that there is something beside God which is eternal, uncreated, infinite, indivisible, immutable. Both of which may justly be thought pernicious and absurd notions.'<sup>1</sup>

Although Berkeley claimed that the chief merit of his rejection of Newtonian space was theological, the principal arguments which he formulated in support of his idea were not. In *De Motu* his earlier discussion was considerably expanded. In Paragraph 66 he sets forth in clear, concise language the rules which he advocates for studying the nature of motion: (1) to distinguish mathematical hypotheses from the natures of things; (2) to beware of abstractions; (3) to consider motion as something sensible or at least imaginable, and to be content with relative measures. If we adopt these rules 'all the famous theorems of the mechanical philosophy by which the secrets of nature are unlocked, and by which the system of the world is reduced to human calculation, will remain untouched; and the study of motion will be freed from a thousand minutiae, subtleties, and abstract ideas'.

The essay falls into three parts: in the first, which comprises some fifty paragraphs, Berkeley discusses the causes of motion; in the second, he considers its nature, arguing (Paragraphs 52 to 65) that it is a relational and not an absolute concept; and the third part (Paragraphs 67 to 72) is devoted to a rather cursory consideration of the problem of the communication of motion from one body to another. Among the interesting points which he makes in the first part are that force regarded as a quality distinct from motion is 'nothing but an occult quality' (Paragraph 5); that attraction was 'introduced by Newton, not as a true, physical quality, but only as a mathematical hypothesis' (Paragraph 17); and that inertia is *not* something inherent in a single, isolated body, 'for there is nothing active' in the typical

<sup>1</sup> G. Berkeley, *A Treatise Concerning the Principles of Human Knowledge*, Section 117

qualities of a body, such as impenetrability, extension and figure, 'which can in any way be understood as the source and principle of motion' (Paragraph 22).

Berkeley's greatest achievement in this essay, however, is his discussion in Paragraphs 52 to 65 of Newton's concepts of absolute space and absolute motion. The fundamental dilemma in which Newton got himself entangled concerning the identification of absolute space and of the ultimate point of reference with respect to which motion should be determined did not exist for Berkeley. In his view there was no need to invoke—indeed there was a definite advantage in not invoking—the idea of space as something existing apart from our perception of bodies. Every place was relative, every motion was relative. If all bodies were destroyed we should be left with mere nothing, for all the attributes assigned to empty space are immediately seen to be privative or negative, except its extension. But this, when space is literally empty, cannot be divided or measured and so it too is effectively nothing. If there existed one 'globe' alone then no motion could be assigned to it; if two 'globes' then 'whatever we may understand by the application of forces, a circular motion of the two globes round a common centre cannot be conceived by the imagination'. At this point Berkeley makes his most penetrating contribution to the problem. Instead of going on to point out that three 'globes' would permit us to consider motions in a plane and that four 'globes' would be required before we could conceive motion in three dimensions, he argues: 'But suppose that the sky of the fixed stars is created; suddenly from the conception of the approach of the globes to different parts of that sky the motion will be conceived.'

We have seen that Newton tended to follow Kepler in his identification of the 'centre of the world', but unlike Kepler he believed that the Sun was 'in continual agitation' under the gravitational attraction of the planets and so could not strictly be taken for the immovable centre. Instead 'a fixed point must be chosen from which the centre of the Sun recedes least, and from which it would recede yet less if the body of the Sun were denser and greater, and therefore less apt to be moved'.<sup>1</sup> As previously mentioned, he identified this fixed point with the centre of gravity of the solar system. In Newton's view not only were the stars scattered throughout infinite space, but

<sup>1</sup> I. Newton, *Mathematical Principles of Natural Philosophy and System of the World* (transl. F. Cajori), Berkeley (Calif.), 1947, p. 420

## BERKELEY'S PHILOSOPHY OF MOTION

like the Sun they were subject to gravitation and therefore presumably also 'in continual agitation' and consequently could not readily provide a fixed point of reference. Berkeley partially reverted to the older view of Copernicus and Ptolemy, but he speaks of the *sky* of fixed stars ('Supponamus deinde coelum fixarum creari'), not of the sphere as they did. I imagine that he thought the stars could be regarded as the (relatively) fixed lattice points of a space of reference. How up-to-date his astronomical knowledge was in 1720 I do not know, but until 1718 no one brought forward any *observational* evidence which conflicted with the traditional belief that (relative to each other) the stars were fixed. In that year Halley discovered that Sirius, Arcturus and Aldebaran differed appreciably from their positions as recorded in the catalogue compiled in antiquity by Hipparchus. The obvious explanation was that these stars had moved against the general background of the other stars. The problem of determining an 'ultimate' background is now recognised to be one of successive approximation, but Berkeley appears to have grasped the essential point that no *one* star should be regarded as more favoured than any other and hence that reference should be made to the framework of *all*.

For a century and a half Berkeley's suggestion does not appear to have been pursued further. It was revived by Ernst Mach in the latter part of the nineteenth century. In his classic *Science of Mechanics* he argued that the motion of a body K can only be estimated with reference to other bodies A, B, C, but since we are restricted to no one *definite* body as origin of reference the conviction arose that these bodies are indifferent generally.

It might be, indeed, that the isolated bodies A, B, C, . . . play a merely collateral role in the determination of the motion of the body K, and that this motion is determined by a *medium* in which K exists. In such a case we should have to substitute this medium for Newton's absolute space. Newton certainly did not entertain this idea. Moreover, it is easily demonstrable that the atmosphere is not this motion-determinative medium. We should, therefore, have to picture to ourselves some other medium, filling, say, all space, with respect to the constitution of which and its kinetic relations to the bodies placed in it we have at present no adequate knowledge. . . . The comportment of terrestrial bodies with respect to the earth is reducible to the comportment of the earth with respect to the remote heavenly bodies. When, accordingly, we say that a body preserves unchanged its



direction and velocity *in space*, our assertion is nothing more or less than an abbreviated reference to the *entire universe*.<sup>1</sup>

Since, in this context, Mach made no reference to Berkeley's *De Motu*, the belief became widespread that Mach originated this line of thought so that Einstein, when generalising the idea in the form of a correlation of the laws of nature with the distribution of matter in the universe, considered it appropriate to coin the term 'Mach's principle'. Nevertheless, despite his more careful and detailed treatment, Mach was anticipated by Berkeley in all essentials, including the critical discussion of Newton's analysis of rotational motion.

We have seen that Newton's greatest difficulty in isolating absolute space from other spaces was the determination of a fundamental origin of reference. Given such an origin we can conceive a multitude of spaces rotating around it, but Newton believed that empirical phenomena enabled us to decide which of all such spaces is absolute. Against this apparently impregnable bulwark of the Newtonian philosophy of space and motion Berkeley directed his fire of criticism. In Paragraphs 60 and 62 of *De Motu*, he pointed out that the motion of the bucket in Newton's experiment is only apparently and not truly circular 'as that term is conceived by those who define the true places of bodies by the parts of absolute space, since it is strangely compounded of the motions, not alone of the bucket . . . but also of the daily motion of the earth round her own axis, of her monthly motion round the common centre of gravity of earth and moon, and of her annual motion round the sun'. He concluded that the phenomena cited by Newton merely indicate rotation relative to the other bodies of the universe and that it is unnecessary to introduce the idea of absolute space which in no way affects the senses and 'is quite useless for the distinguishing of motions'. In his discussion Mach made the same point, remarking that the only experimental test that could be imagined for the falsification of the idea that rotational motion is relative (with respect to the universe as a whole) would be to compare Newton's experiment as he performed it with one in which the bucket is left undisturbed and the universe is made to rotate around it. The test is impossible to carry out and so we are not compelled to accept Newton's interpretation of his experiment.

The problem of relating our dynamical concepts to our ideas of

<sup>1</sup> E. Mach, *The Science of Mechanics* (transl. T. J. McCormack), La Salle (Ill.), 1942, pp. 283 ff.

## BERKELEY'S PHILOSOPHY OF MOTION

the universe as a whole still remains with us. No longer do we refer in this context to the system of stars, whether 'fixed' or otherwise, but rather to the system of star-systems or galaxies. Further discussion would take us into much deeper waters than those into which Berkeley plunged, but we should not underestimate the power and originality of his contribution in an age otherwise noteworthy for its complacency in the problem.

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# A NOTE ON THE DESCRIPTIVE CONCEPTION OF MOTION IN THE FOURTEENTH CENTURY

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READERS of *De Motu* may be surprised at the extent to which Berkeley anticipated many of the most important points in Mach's critique of Newtonian mechanics.<sup>1</sup> The central doctrine advocated by both writers was summarised by Berkeley in the following rules for 'determining the true nature of motion : (1) to distinguish mathematical hypotheses from the natures of things ; (2) to beware of abstractions ; <sup>2</sup> (3) to consider motion as something sensible, or at least imaginable ; and to be content with relative measures' (§ 66). 'It is not', he wrote, '... the business of physics or mechanics to establish efficient causes, but only the rules of impulsions or attractions, and, in a word, the laws of motions, and from the established laws to assign the solution, not the efficient cause, of particular phenomena' (§ 35). 'The physicist studies the series or successions of sensible things, noting by what laws they are connected, and in what order' (§ 71). So he rejected Newton's concepts of absolute space and motion, retaining in physics only 'space comprehended or defined by bodies, and therefore an object of sense, ... called relative, apparent, vulgar space' (§ 52) and the corresponding relative motion.

In all this we can see the development of what has come to be called the 'descriptive', as opposed to the 'essentialist' conception of scientific theories.<sup>3</sup> The purpose of this note is to draw attention to a parallel descriptive conception of motion enunciated in the fourteenth century in criticism not of Newtonian, but of Aristotelian physics. I have discussed the history of this descriptive conception of

<sup>1</sup> See the articles by Popper and Whitrow in this issue of this *Journal*. I have used *The Works of George Berkeley*, ed. A. A. Luce and T. E. Jessop, Vol. IV, London and Edinburgh, 1951, containing the Latin text of *De Motu* with Luce's translation.

<sup>2</sup> By 'abstractions' he meant 'force, gravity, attraction, and terms of this sort ... useful for reasonings and reckonings about motion and bodies in motion, but not for understanding the simple nature of motion itself or for indicating so many distinct qualities' (§ 17).

<sup>3</sup> Cf. K. R. Popper, 'The Poverty of Historicism', *Economica*, London, 1944, N.S. II, 94-95 ; J. O. Wisdom, *Foundations of Inference in Natural Science*, London, 1952. See above, p. 29, n.1.



## MOTION IN THE FOURTEENTH CENTURY

scientific theories down to Newton in my *Robert Grosseteste and the Origins of Experimental Science, 1100-1700* (Oxford, 1953). Here I make no attempt to discover what Berkeley may have known of fourteenth-century authors, but it may, I think, be presumed that so acute and scholarly a critic would not intentionally have thrown contributions so similar to his own into the common ragbag of 'the obscure subtlety of the schoolman, which for so many ages, like some dread plague, has corrupted philosophy' (§ 40).

Aristotle's explanation of local motion was based on two essential assumptions: (1) that every moving body required a continuous efficient cause to maintain its velocity;<sup>1</sup> and (2) that bodies had natural motions determined by their position in the universe in relation to the sphere of the element predominating in their composition. Thus, on the earth's surface, a stone, composed predominantly of earth, had a natural motion downwards towards the centre of the earth, whereas flames moved naturally upwards towards the sphere of fire. Since there was only one universe and the earth was at its centre, these directions were said to be 'absolute'.

Aristotle's first assumption raised special problems when it came to explaining the motion of a projectile.<sup>2</sup> What kept a stone in motion after it had left the hand of the person who threw it? Explanations were offered both in terms of the action of the air through which the stone passed, and in terms of various conceptions of *virtus* or power imparted to the stone itself by the hand. This second type of explanation reached its final form in the first half of the fourteenth century in the French physicist, Jean Buridan's conception of *impetus*—which may perhaps be described as something analogous to Newtonian momentum considered as an efficient cause. It was this conception of the need to postulate an efficient cause of continuous motion that was singled out by the chief fourteenth-century critic of Aristotle's first assumption, William of Ockham.

Making use of his principle of economy, or Razor: 'it is futile to use more entities when it is possible to use fewer', Ockham asserted

<sup>1</sup> In § 43 Berkeley cites 'definitions of Aristotle and the school-men'.

<sup>2</sup> For the history of this problem see P. Duhem, *Études sur Léonard de Vinci*, 3<sup>e</sup> série, Paris, 1906-13; A. Koyré, *Études Galiléennes* (*Actualités scientifiques et industrielles*, Nos. 852-854), Paris, 1939; A. R. Hall, *Ballistics in the Seventeenth Century*, Cambridge, 1952. See also E. A. Moody, 'Galileo and Avempace. The history of the Leaning Tower Experiment', *Journal of the History of Ideas*, 1951, 12, 163, 375; and A. C. Crombie, *Augustine to Galileo*, London, 1952.

in his *Tractatus de Successivis*, I, 'De Motu',<sup>1</sup> that there was no need to postulate any external or internal agent or efficient cause to maintain the motion of a body. We need only consider the change of position of the observable moving body in relation to other bodies.

That without such an additional thing we can save motion and everything that is said about it is made clear by considering the separate parts of motion. For it is clear that local motion can be conceived as follows: positing that the body is in one place and later in another place, thus proceeding without rest or any mediating thing other than this body and agent which moves, we have local motion truly; therefore it is futile to postulate such other things.

The particular 'essentialist' conceptions from which Ockham was trying to extricate the concept of motion are shown plainly in a well-known passage in his *Super Quattuor Libros Sententiarum Annotationes*, where he again asserts in effect that the student of motion 'studies the series or successions of sensible things'.

I say therefore that the moving thing in such a motion [i.e. projectile motion], after the separation of the moved body from the first projector, is the moved thing itself, not by reason of any power in it; for this moving thing and the moved thing cannot be distinguished. If you say that a new effect has some cause and local motion is a new effect, I say that local motion is not a new effect . . . because it is nothing else but the fact that the body in motion is in different parts of space in such a manner that it is not in any one part, since two contradictories are not both true.<sup>2</sup>

To describe motion we can say: "this body is now at A and not at B", and later it will be true to say: "this body is now at B and not at A", so that contradictories are successively made true'.<sup>3</sup>

Criticism of Aristotle's second dynamical assumption, namely that in effect space had a structure with absolute favoured directions for bodies of different kinds, was made contemporaneously. 'Mathematical space', a void extended in three dimensions, had been distinguished in the thirteenth century by Robert Grosseteste and Roger Bacon from the real space through which bodies moved and natural influences like magnetism and light were propagated.<sup>4</sup>

<sup>1</sup> Ed. P. Boehner (Franciscan Institute Publ. 1), New York, 1944, p. 45

<sup>2</sup> The Latin text of this passage is quoted by A. Maier, *Zwei Grundprobleme der scholastischen Naturphilosophie*, Roma, 1951, p. 157. See my *Robert Grosseteste*, pp. 171-177.

<sup>3</sup> *Tract. Successivis*, III, 'De Tempore', ed. Boehner, p. 122

<sup>4</sup> See my *Robert Grosseteste*, pp. 99, 106, 145-146.

## MOTION IN THE FOURTEENTH CENTURY

For vacuum rightly conceived of is merely a mathematical quantity extended in the three dimensions, existing *per se* without heat and cold, soft and hard, rare and dense, and without any natural quality, merely occupying space, as the philosophers maintained before Aristotle, not only within the heavens, but beyond.<sup>1</sup>

In the second half of the fourteenth century the French mathematician, Nicole Oresme, in his French commentary on Aristotle's *De Caelo*, analysed the whole conception of absolute and relative directions in space, and concluded that right, left, before, behind, up, down were not 'in the sky absolutely and really distinct, but only relatively, as it is said';<sup>2</sup> that is, relatively to an arbitrary standpoint taken by a person on the earth. He went on to show that the only motion that could ever be observed was relative motion.

I assume that local motion cannot be observed except in so far as a body may be seen to change its position (*avoir autrement*) in relation to another body. To illustrate this (*pour se*), if a man is in a boat A, moving very smoothly (*souef*), fast or slowly, and he can see nothing outside [his own boat] except another boat B, moving in exactly the same manner as the boat A in which he is, I say that it will seem to this man that neither of the boats is moving. If A is at rest and B is moving, it will seem to him that B is moving; and if A is moving while B is at rest, it will seem to him just as before that B is moving; so if A was at rest for an hour and B moved, and then in the next hour, *e converso*, A moved and B remained at rest, this man would not be able to perceive this change or variation, but it would seem to him all the time that B was moving; and this is evident from experience. . . . It would seem to us all the time that the place where we are was at rest and that the other always moved, just as it seems to a man in a moving boat that the trees outside are moving. Similarly, if a man was on the sky, supposing that he was moving with daily motion . . . it would seem to him that the earth was moving with daily motion, just as the sky seems to be to us on the earth. Similarly if the earth was moving with daily motion and the sky was not, it would seem to us that the earth was at rest and that the sky was moving.<sup>3</sup>

Anything that might have remained of Aristotle's absolute favoured directions and natural motions in space Oresme removed by adopting

<sup>1</sup> Roger Bacon, *Opus Maius*, Pars V. i, Dist. ix, Cap. 2, ed. Bridges, Oxford, 1897, 2, p. 67

<sup>2</sup> Maistre Nicole Oresme, *Le Livre du ciel et du monde*, fol. 86d, ed. A. D. Menut and A. J. Denomy, *Mediæval Studies*, Toronto, 1942, 4, 194. Cf. fol. 37c-d, *ibid.*, 1941, 3, 242; 1943, 5, 262.

<sup>3</sup> fols. 138d-139b; *ibid.* 1942, 4, 272



a relative conception of gravity. For about a century much attention had been given to the problems whether space could be infinite and could contain other 'worlds' besides that of the earth and its accompanying heavenly bodies. Oresme followed those who argued that God, by his infinite power, could create an infinite space and as many worlds as he chose.<sup>1</sup> Movements were then produced by gravity only relative to a particular world; there was no absolute direction of gravity applying to all space. The 'natural ordering' of heavy and light bodies in a given world was 'such that all the heavy bodies, as far as possible, were in the middle of the light ones, without determining any unmoving place (*lieu immobile*) for them'.<sup>2</sup> He went on to discuss arguments in favour of constructing an astronomical system on the assumption that the earth was in motion and the heavens at rest, asserting the advantage that 'all the effects we see can be made and all appearances saved by a small operation, namely the daily rotation of the earth, which is very small compared with that of the sky, without the multiplication of operations so diverse and outrageously large'.<sup>3</sup> If, at the end of his arguments, Oresme still held to the belief that 'there never was and never will be but only one corporeal world',<sup>4</sup> and continued to use the Ptolemaic system,<sup>5</sup> this does not detract from the acuteness of his analysis of the problem of what was observable in the motions of bodies in space.

The seventeenth-century system of mechanics was constructed by carrying out the 'descriptive' programme implied by Ockham and Oresme by means of new mathematical techniques, not precisely undreamt of, but certainly undeveloped, in the fourteenth century. Galileo described this programme in a well-known passage in his *Two New Sciences*, Day Three, when, in discussing falling bodies, he declared that he did not intend 'to investigate the cause of the acceleration of natural motion, concerning which various opinions

<sup>1</sup> fols. 260; 1941, 3, 233, 243; fols. 31b, 38c. 'Et donques hors le ciel est une espace wide incorporelle, d'autre maniere que n'est quelconque(s) espace pleine et corporelle, tout aussi comme la duracion temporelle, meismes qui seroit perpetuelle . . . Item, cest espace dessus dicte est infinie et indivisible et est le immensité de Dieu et est Dieu meismes, aussi comme la duracion de Dieu appelee eternité est infinie et indivisible, et Dieu meisme. . . .' (fol. 39a-b).

<sup>2</sup> fol. 37 c-d; *ibid.* p. 242. This was the so-called Pythagorean theory of gravity.

<sup>3</sup> fol. 143d; *ibid.* 1942, 4, 278; see also fols. 92-95, pp. 203-209. Cf. my *Augustine to Galileo*, pp. 255-257; *Robert Grosseteste*, pp. 202-203.

<sup>4</sup> fol. 39c; ed. Menut and Denomy, 1941, 3, 244

<sup>5</sup> fol. 70, *ibid.* 4, 169; fols. 117-122; *ibid.* pp. 240-249; fol. 153, p. 292

## MOTION IN THE FOURTEENTH CENTURY

have been expressed by various philosophers. . . . It is not really worth while'; he intended only 'to investigate and demonstrate some of the properties of accelerated motion, whatever the cause of this acceleration may be'.<sup>1</sup> The result was his law, now written  $v = \frac{1}{2}gt^2$ , which he proved by the same theorem as Oresme.<sup>2</sup> The burden of most of Newton's often-quoted remarks about hypotheses was to assert the same descriptive programme.<sup>3</sup> Berkeley's criticisms of Newton were but the extension of a conception of scientific explanation which already had a long and fruitful history.

<sup>1</sup> *Le Opere di Galileo Galilei*, ed. naz., Florence, 1898, 8, 202

<sup>2</sup> See Duhem, *Études sur Léonard de Vinci*, 3, 394-395, 574-583; cf. my *Augustine to Galileo*, pp. 261-263.

<sup>3</sup> On Galileo and Newton, see my *Robert Grosseteste*, pp. 303-309, 315-318.

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# BERKELEY'S *QUERIST* AND ITS PLACE IN THE ECONOMIC THOUGHT OF THE EIGHTEENTH CENTURY<sup>1</sup>

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## I

IN 1734, when Berkeley returned to Ireland as Bishop of Cloyne, that island had for some time been sunk deep in a combination of conditions which economists would today distinguish as those of a depressed economy, a distressed area, an under-developed country, and an exploited colony. Straightaway, Fraser tells us, 'the social condition of Ireland, especially of the aboriginal population, began to engage Berkeley's thoughts'.<sup>2</sup> He set out to combat the economic distress around him both by practical measures in his own diocese (one in which it was then unusual for the Bishop even to reside),<sup>3</sup> and by composing a plan of, and summons to, national action in his pamphlet *The Querist*, published in three instalments in 1735, 1736 and 1737, as well as in a London edition at about the same time. Bishop Berkeley defended his concern with economic problems in his foreword to the second edition: 'I anticipate the same censure on this, that I incurred upon another occasion, for meddling out of my profession. Though to feed the hungry and clothe the naked, by promoting an honest industry, will, perhaps, be deemed no improper

<sup>1</sup> I am very grateful to Dr J. O. Wisdom of the London School of Economics for advising me about, and actually lending me copies of, a number of writings on Berkeley, including some most helpful unpublished work of his own.

<sup>2</sup> Fraser, *Works of George Berkeley*, Oxford, vol. 4, p. 242

<sup>3</sup> Vide E. D. Leyburn, 'Bishop Berkeley: "The Querist"', *Proceedings of the Royal Irish Academy*, Dec. 1937, who quotes a letter of Berkeley's of 1737: 'Our spinning school is in a thriving way. The children begin to find a pleasure in being paid in hard money; which I understand they will not give to their parents, but keep it to buy clothes for themselves. Indeed I found it difficult and tedious to bring them to this; but I believe it will now do. I am building a workhouse for sturdy vagrants, and design to raise about two acre of hemp for employing them. Can you put me in a way of getting hempseed; or does your Society distribute any? It is hoped your flax-seed will come in time. . . .' (Vide Fraser, *op. cit.*, vol. 4, pp. 247-248).



## BERKELEY'S *QUERIST*

employment for a clergyman, who still thinks himself a member of the Commonwealth.'

In its original form *The Querist* consisted of nearly 900 'queries' arranged with an apparently extreme and provocative haphazardness.<sup>1</sup> Altogether the pamphlet presents a thoroughly consistently-reasoned and comprehensive policy to raise the Irish economy and the standard of life of the people from their depressed and dependent state. But the first aim of *The Querist* was to stimulate thought and discussion and to 'make his countrymen think'—as he puts it—rather than to lay down a rigid programme: 'The Querist, indeed, only puts questions, and offers hints, not presuming to direct the wisdom of the legislature.' (Introduction to *Queries Relating to a National Bank, Extracted from the Querist*, 1737.) Berkeley was hopeful that thought and public discussion could lead to enlightened government action which would radically improve economic conditions and, above all, 'feed the hungry and clothe the naked'.

Although the result is consistent and well-knit, Berkeley's programme is rather built upwards out of particular practical proposals suggested by the closely-observed problems around him, than deduced downwards from a set of formulae or generalisations. Berkeley did not have at his disposal or seek to construct an organised orthodox set of definitions, assumptions, or 'tools of thought'. He simply brought to bear his great mind and deep sympathies on a set of practical commonsense proposals, introducing almost as a by-product the minimum of theory and analysis necessary for supporting and explaining his programme. Today *The Querist* must be read (as must, of course, most eighteenth-century and earlier economic writings) in the spirit in which Berkeley himself had asked that even his profounder philosophical arguments should be approached: 'I wish . . . , our opinions were fairly stated and submitted to the judgment of men who had plain common sense, without the prejudices of a learned education.'<sup>2</sup> This invocation of 'plain common sense' might well have appealed to the main Scottish architects of classical political

<sup>1</sup> I have used Hollander's reprint of the first (1735-37) edition except in one or two (specified) places. A second edition was published in 1750 omitting many of the queries of the first edition, mainly the sharper and more sarcastically worded ones. The second edition added only a few queries not in the first. The differences in the two editions are important for Berkeley's character in his later years but have no significance for his economic arguments.

<sup>2</sup> Fraser, op. cit., vol. I, p. 332 (The Third Dialogue between Hylas and Philonous)

economy, Hutcheson, Smith, and, to some extent, Hume. But, unlike Hutcheson and Smith, Berkeley addressed himself to a particular practical problem and did not approach political economy as one department of a comprehensive 'philosophical' system of moral studies. On the other hand, though going straight for topical, practical issues, Berkeley was incomparably freer from the business, financial, or party-political interests which pervaded so many of the economic pamphleteers of that (and many another) period. Few economic writers can ever have combined to a higher extent the virtues of immediate practical relevance and profound intellectual disinterestedness.

There is little or nothing in the way of specific references by Berkeley to the background of writings and ideas on political economy which may have helped him in building up the programme of *The Querist*. But however much or little Berkeley may have read and marked the writings of his contemporaries and recent predecessors, most of his leading ideas are to be found in the English economic thought of the previous half-century, in particular in the works of Petty (1662 et seq.), Barbon (1690), North (1691), and John Law (1705). The rather staccato interrogative procedure of *The Querist* may have been suggested by Petty's monetary tract *Quantulumcunque* (1682). There had also been one or two noteworthy pamphlets dealing specifically with Irish monetary problems.<sup>1</sup> It is clear, too, that in the course of his European and American travels, Berkeley had studied closely the details of banking institutions and the acute contemporary monetary problems in different countries. He had obviously followed with deep interest the course of John Law's experiment in Paris (1716-20) with his government-controlled Banque Royale and its paper currency, but had not allowed its disastrous practical conclusion to distract him from the theoretical penetration and soundness contained in some of Law's ideas.

Apart from *The Querist*, the only other of Berkeley's works bearing at all on political economy is his short *Essay towards preventing the Ruine of Great Britain* (1721). His message here is mainly a moral one, directed against the misuse of economic freedom which had produced the South Sea Bubble. But his economic proposals for

<sup>1</sup> For example, Thomas Prior, a close friend of Berkeley, had published his *Observations on Coin* in 1729, and there had been Isaac Newton's *Representation of* 1712. Both these were republished by McCulloch in his *Old and Scarce Tracts on Money*.

## BERKELEY'S 'QUERIST'

Britain, though very briefly stated, are, in general outline, very similar to those later proposed for Ireland in *The Querist*, and we shall make some quotations from the *Essay* as well as from *The Querist*.

### 2

The main features of Irish economic conditions with which Berkeley was confronted, and which he sought to remedy, can be gathered from the suggestions of *The Querist*, as one goes through it. Monetary conditions were primitive, defective, and confused in the extreme.<sup>1</sup> Moreover, the currency change of 1701 had had the effect of discouraging exports, already hard hit by English policy, notably by the prohibition on Irish woollen exports in 1699, 'one of the most infamous statutes that ever disgraced a legislature', as Arthur Young subsequently described it.<sup>2</sup> Apart from these particular aggravations there was much in the conditions Berkeley contemplated in Ireland in the 1730s which seems closely similar in general outline to those assumed by many seventeenth- and eighteenth-century economists elsewhere, both in Britain and France. In addition to facing primitive monetary difficulties, and sharp but persistent strains on the balance of payments in conditions of intense national rivalry, Berkeley was, above all, confronted with chronic and heavy unemployment. It certainly did not occur to Berkeley, in Ireland in 1735, as coming within the bounds of practical policy, to consider the setting up and supervision of institutional arrangements which could conceivably render self-regulating at satisfactory levels, both the internal level of aggregate demand, supply and employment, and, externally, the balance of payments. Only, it seemed to Berkeley, by a comprehensive and complex programme of State action combining labour legislation with fiscal, monetary, and public-works policies, could a high level of employment and external solvency be secured for Ireland.

There is one further condition facing Berkeley and seventeenth- and eighteenth-century economists generally, which marks another vital difference between his assumptions and those which later became more orthodox. From his close knowledge of labour conditions he had to assume that generally the supply of labour was very inelastic

<sup>1</sup> Cf. J. Johnston, 'Commercial Restriction and Monetary Deflation in 18th Century Ireland', *Hermathena*, 1939, 28, 79 ff.

<sup>2</sup> Cf. Lipson, *Economic History of England*, London, 1931, vol. 2, p. 204



once a certain traditional and very low standard of living had been earned, scraped, or begged. More moralistically, one might say—as contemporaries often did—that the Irish people were bone idle, preferring as much leisure as possible to regular work, or that any rise in their daily wage-rates was likely to be taken out in higher absenteeism per week. This is how Berkeley put it: 'Whether the bulk of our Irish natives are not kept from thriving by that cynical content in dirt and beggary which they possess to a degree beyond any other people in Christendom?' (i. 19)

'The oddity of Ireland is Berkeley's favourite theme in *The Querist*', an Irish biographer has stated.<sup>1</sup> But on this point, at any rate, it would be quite unnecessary to invoke any special Irish oddity. A few years later Dean Tucker was complaining that *English* labourers 'are as bad as can be described; who become more vicious, more indigent and idle in proportion to the advance of wages and the cheapness of provisions. . . . This is the ruin of all our trade, too many there are who will not accept work one part of the week but on such terms as will enable them to live in vice and idleness the rest.' Stripped of all moralising, this bare fact about the prevalent shape of the supply curve of labour, whether Irish or British, is asserted by one well-informed observer after another in the seventeenth and eighteenth centuries, from Petty and Defoe to Arthur Young and Sir James Steuart, and of course it can be met with in many parts of the world today not yet penetrated with the urban-industrial gospel of regular work, and not stimulated in its appetites by capitalist advertising or government propaganda and compulsion.<sup>2</sup>

<sup>1</sup> cf. J. M. Hone and M. M. Rossi, *Bishop Berkeley*, London, 1931, p. 200

<sup>2</sup> This problem is extensively discussed and documented in the remarkable study, *The Position of the Laborer in a System of Nationalism*, by E. S. Furniss, Boston, 1920, pp. 234-235: 'This tendency, represented by the "back-turning supply curve" for labor, is recognized by all students of economics, but in *laissez-faire* doctrine it is recognized only as the exception and not as the rule. . . . We find a widespread agreement among the writers of Mercantilist England . . . That the effect of increased prosperity among the English laborers was not a better, but an inferior, quality of industry, not more, but less, labor we must conclude from the almost unanimous testimony of contemporary writers. . . . The wants of the average individual to-day are but loosely restricted by custom and tradition . . . Among traditionalistic peoples, on the contrary, where a rigid standard of living, embracing not much more than the necessities of physical subsistence, obtains, any increase in wages will result in an immediate diminution in labor hours.' Cf. also Cunningham, *Growth of English Industry and Commerce, The Mercantile System*, 5th ed., Cambridge, 1925-27, p. 566, who refers to 'the repeated charges of idleness . . . brought

## BERKELEY'S QUERIST

This stubborn fact of a 'perverse' supply curve of labour is an extremely obstructive and irritating one for those anxious to promote a programme of economic development and expansion, whatever their political assumptions. Many of the seemingly odd 'interventionist' suggestions, sometimes harsh, sometimes optimistic, to be found in seventeenth- and eighteenth-century writings, arise from this general fact about the supply curve of labour—that an increase in wage-rates will be followed by a *fall* in the quantity of labour forthcoming per week—a fact which the classical economists later treated mainly as an exception in their theorising, though some of the details of the Poor Law Reform may have been attributable to it. It may well be that by the beginning of the nineteenth century the disciplines of urban industrialism, aided by the removal of possibilities for self-employment, had twisted the more rigid 'perversity' out of the general labour supply curve. Anyhow, in the classical system it seems generally to have been assumed that a rise in wages would be followed by an increase in the supply of labour, and vice versa, and finally we have Marshall laying it down: <sup>1</sup> 'Subject to . . . qualifications, it is broadly true that the exertions which any set of workers will make rise or fall with a rise or fall in the remuneration which is

against the poor', and quotes Defoe: 'I make no difficulty to promise on a short summons to produce above a thousand families in England, within my particular knowledge, who go in rags and their children wanting bread, whose fathers can earn their 15 to 25 shillings per week but will not work, who may have work enough but are too idle to seek after it, and hardly vouchsafe to earn anything but bare subsistence and spending money for themselves.' See also Heckscher, *Mercantilism*, London, 1935, vol. 2, p. 165, who, discussing what he describes as the doctrine of 'the economy of low wages', cites as an upholder Sir William Petty, 'who had no private interests to bias him in favour of employers, and who had a better scientific culture than most writers of the Restoration period'. Heckscher concludes that 'according to the statements of many mercantilist writers the more people were paid, the less they worked'. Frances Hutcheson agreed with these 'mercantilists' in holding that 'if a people have not acquired an habit of industry, the cheapness of all necessities of life rather encourages sloth' (*System of Moral Philosophy*, 1755, vol. 1, p. 318). But his pupil Adam Smith significantly altered the emphasis, laying it down that 'the liberal reward of labour' increases both the numbers and the industry of the common people: 'Some workmen, indeed, when they can earn in four days what will maintain them through the week, will be idle the other three. This, however, is by no means the case with the greater part.' (*Wealth of Nations*, Everyman ed., p. 73) Hume is non-committal, but notes the phenomenon of more labour being forthcoming in years of scarcity than in those of plenty (see *Essay on Taxes*).

<sup>1</sup> *Principles*, 8th ed., p. 142

offered to them.' It is also important that for the construction of self-adjusting models it is, *purely theoretically*, much more convenient to assume that a rise (or fall) in wages increases (or decreases) the supply of labour, rather than the reverse, which seemed the more realistic (though both theoretically and practically the far more awkward) assumption, for many seventeenth- and eighteenth-century economists.<sup>1</sup>

## 3

We may look, then, at *The Querist* as a programme for building up and maintaining a high and stable level of employment, and for raising the standard of living of the mass of the people, in the face of chronic unemployment, a 'perverse' supply curve of labour, vast inequalities in the distribution of wealth, and catastrophically primitive monetary arrangements. In spite of the great differences in assumptions, Berkeley's programme falls very easily under the three heads

<sup>1</sup> Furniss, *op. cit.*, mentions many devices used or recommended for combating the 'perverse' labour supply curve in the eighteenth century. In 1946-47, post-war shortages and rigidities produced for a time a similar temporary perversity at a critical point of the British economy, in the coal-mining industry. It encourages one to believe in the hypothesis of regularities in economic behaviour when one notices how closely all the eighteenth-century measures were followed in various proposals, some rather pathetic, discussed at the time (except for the proposal of a direct cut in money wages institutionally impossible in 1946-47): Berkeley's and North's idea of stimulating new wants (p. 178) was revived in proposals to rush special supplies of 'incentive goods' such as nylons and tinned pineapple to the shop-windows of mining towns: Tucker's proposed immigration of foreign protestants (p. 35) was repeated in the proposed importation of masses of Italians: the same purpose of reducing real wages by taxing food, discussed by John Law, and with reservations by Hume (p. 136), was to be achieved at the later date by reducing food subsidies: the suppression of fairs, festivals and distractions from regular work (p. 150) had the milder equivalent of cutting down midweek soccer and racing. But in the twentieth century this 'perversity' could be only a temporary *curiosum* soon dissolving before the massive long-run disciplines of modern advertising, government propaganda and the emulative spirit, undeveloped, of course, in the eighteenth century. (The page references indicate where Furniss discusses the eighteenth century policies.) As regards pure theoretical convenience, the devoted attachment shown in recent years to the idea of the *interest* rate as an effective and satisfactory (if not optimal) equilibrator of the demand and supply of the other main contractually-rewarded factor of production, indicates (whether or not the attachment is justified) how convenient has been the assumption, impossible in the eighteenth century, that the *wage* rate was a smooth instrument of satisfactory equilibration in an expanding economy.



under which any such modern programme could conveniently be considered :

- (a) the supply curve of labour and the labour market ;
- (b) fiscal policy and problems of the balance of payments ;
- (c) monetary institutions and policy.

(a) *The labour market* : Human wants are the driving force behind an active and prosperous economy, and the most liberal way of countering a ' perverse ' supply curve of labour is to stimulate the desire for a higher standard of living and for new commodities (or ' incentive goods ' ) not entering into the traditionally-accepted very low consumption habits, say a pair of shoes, rather better housing, and perhaps even a little soap or sugar :

Whether the creating of wants be not the likeliest way to produce industry in a people ? And whether if our peasants were accustomed to eat beef and wear shoes they would not be more industrious ? (I. 20)

At very low traditional levels there is a vicious circle of poverty and idleness which has to be broken through if the standard of living of the people and the wealth of the country is to be increased :

Whether nastiness and beggary do not, on the contrary, extinguish all such ambition, making men listless, helpless and slothful ? (I. 66)

Whether comfortable living doth not produce wants, and wants industry, and industry wealth ? (I. 113) <sup>1</sup>

Whatever the possibilities of raising wants and standards without the blessed weapons of modern commercial advertising and

<sup>1</sup> Cf. Furniss, op. cit., p. 178 : ' There was one small group of writers who took a more advanced position in regard to the standard of living than that attained by the mass of social observers. . . . This group comprised Dudley North, George Berkeley, and David Hume.' Furniss quotes North (*Discourses*, 1691, p. 27) as follows : ' The main spur to trade or rather to industry and ingenuity is the exorbitant appetites of men which they will take pains to gratify, and so be disposed to work, when nothing else will incline them to it ; for did men content themselves with bare necessities, we should have a poor world ' (cf. Mandeville). Hume wrote : ' It is a violent method and in most cases impracticable to oblige the labourer to toil in order to raise from the soil more than what subsists himself and family. Furnish him with manufactures and commodities and he will do it for himself.' Furniss might have added Sir James Steuart to this group : ' Steuart's idea is that normally the primitive man would not work, unless forced, to produce anything beyond his barest necessities ; but once his taste for luxuries developed, he became willing to work hard to produce a surplus with which to purchase luxuries.' See S. Sen, *Economica*, 1947, vol. 21, on ' Sir James Steuart's General Theory of Employment, Interest and Money '.

government propaganda, Berkeley (like Petty, Steuart, and many other seventeenth- and eighteenth-century economists) holds it to be the inescapable duty of the State to make sure that there is employment for all those ready and eager to take it, an objective which will not otherwise be sufficiently attained by any automatic self-adjusting mechanism. Berkeley does not lay down the frontier line, so important administratively but so difficult to draw precisely, between voluntary and involuntary idleness, and he does not elucidate the condition which subsequently came to be described as 'genuinely seeking work'. But he is perfectly clear that there is vast and chronic *involuntary* unemployment, and he is incensed by the paradox of poverty and unemployment side by side :

Whether there can be a greater reproach, on the leading men and patriots of a country, than that the people should want employment ? (II. 194)

Whether it be not a new spectacle under the sun, to behold in such a climate, and such a soil, after so long a peace, and under such a gentle government, so many roads untrodden, fields untilled, houses desolate, and hands unemployed ? (III. 2)

Whether we are not in fact the only people who may be said to starve in the midst of plenty ? (III. 101)

Local machinery should be set up for getting statistics of unemployment, and organising public works accordingly, on roads, bridges, drainage, public buildings and manufactures :

Whether it may not be right to appoint censors in every parish to observe and make returns of the idle hands ? (II. 197)

Whether a register or history of the idleness and industry of a people would be an useless thing ? (II. 198)

Whether we are apprized of all the uses that may be made of political arithmetic ? (II. 199)

Whether it would be a great hardship if every parish were obliged to find work for their poor ? (II. 200)

Berkeley proposed that it should be impossible to beg a living, or live on public relief without working, and he was prepared to advocate compulsory labour, which he bluntly described as 'temporary slavery' (not as a 'National Labour Corps', or as a 'Corrective Labour Service', to cite some typical neologisms). His proposal was intended for beggars and for common criminals (in the old-fashioned sense of 'criminal' which excludes 'political unreliability', 'thought-crime', etc.):

## BERKELEY'S QUERIST

Whether it would be a hardship on people destitute of all things, if the public furnished them with necessities which they should be obliged to earn by their labour? (II. 213)

Whether temporary servitude would not be the best cure for idleness and beggary? (II. 215)

Whether the public hath not the right to employ those who can not, or who will not, find employment for themselves? (II. 216)

Of course, in judging these proposals one must try, as best one can, to imagine the condition of the Irish poor at this time, and bear in mind the awkward but seemingly indubitable fact of the 'perverse' supply curve of labour, as well as what eventually became the alternatives of starvation or the workhouse in nineteenth century Britain, and prison or the slave-labour camp in the twentieth-century industrialisation of some other parts of the world. Anyhow, we agree with Sampson that Berkeley's 'advocacy of temporary slavery for beggars is not really so startling as might at first appear'.<sup>1</sup> The great ancestor of economic liberalism, Frances Hutcheson himself, laid it down at this period that 'sloth should be punished by temporary servitude at least'.

(b) *Fiscal policy and the balance of payments*: Berkeley's fiscal proposals were aimed at reducing extreme inequalities, helping employment at home, and eliminating the main source of strain on the Irish balance of payments. He completely agreed with Mandeville's argument—expressed also by many other seventeenth- and eighteenth-century economists—that it was the expenditure of the rich that employed the mass of the people:

Whether the industry of the lower part of our people doth not much depend on the expense of the upper? (II. 229)

<sup>1</sup> See Sampson, *Works of Berkeley*, vol. 3, p. 112. In his unrevised and posthumously published book, *The Dilemma of Our Times*, London, 1952, Harold Laski wrote: 'From the ruthless attack on almsgiving, and the equally ruthless promotion of enclosures, it was easy to move to a position where even a man like Bishop Berkeley could propose that the sturdy beggar be taught the habits of industry by undergoing a term of enslavement' (p. 93). Without any further reference to the element of humane equalitarianism in Berkeley's policies this quotation might be very seriously misleading, and we venture to believe that the author would have wanted to revise it before publication. We also venture to believe that Berkeley would have emphatically condemned the heavy gaol sentences for absenteeism and unpunctuality imposed by the labour legislation of certain contemporary governments so oddly supposed to be operating in the interests of the working class. See also Hutcheson, *op. cit.*, p. 319.



But Berkeley, being morally opposed to luxury and to gross inequalities, did not agree with the conclusion which, with rather forced cynicism, Mandeville had suggested as following from this proposition, to the effect that it was therefore inevitably beneficent that the extravagance of the rich and the grossest inequalities of fortune should be given free rein :

Whether necessity is not to be hearkened to before convenience and convenience before luxury ? (I. 63)

Whether to provide plentifully for the poor, be not feeding the root the substance whereof will shoot upwards into the branches, and cause the top to flourish ? (I. 64)

Whether as seed equally scattered produces a goodly harvest, even so an equal distribution of wealth doth not cause a nation to flourish ? (2nd ed., I. 220)

Moreover, luxury goods made up a very large part of Irish imports while several of her main export markets had been prohibited outright by English restrictions. Berkeley did not propose to leave the balance of payments to take its course, but proposed combining sumptuary laws against luxuries with import duties protective of Ireland's solvency :

Whether as our exports are lessened, we ought not to lessen our imports ? And whether these will not be lessened as our demands and these as our wants, and these as our customs or fashions ? (I. 105)  
What the nation gains from those who live in Ireland upon the produce of foreign countries ? (I. 107)

Whether those, who drink foreign liquors and deck themselves and their families with foreign ornaments, are not so far forth to be reckon'd absentees ? (I. 110)

Whether the dirt, and famine, and nakedness of the bulk of our people might not be remedied even though we had no foreign trade ? And whether this should not be our first care, and whether, if this were once provided for the conveniences of the rich would not soon follow ? (I. 112)

The expenditure of the rich could and should be so canalised as to give employment at home (for example, on houses and furniture), and solvency externally :

What would be the consequences, if our gentry affected to distinguish themselves by fine houses rather than fine clothes ? (II. 230) <sup>1</sup>

<sup>1</sup> Barbon had made a similar point as to the beneficence of expenditure by the rich on building 'since the erection of houses employs a greater number of people than feeding and clothing'. See Furniss, *op. cit.*, p. 57, and Barbon, *Discourse*, 1690, p. 62.

Whether building would not peculiarly encourage all other arts in this kingdom? (II. 232)

Whether by these means much of that sustenance and wealth of this nation which now goes to foreigners would not be kept at home and nourish and circulate among our own people? (II. 236)

Whether as industry produced good living, the number of hands and mouths would not be increased, and in proportion thereunto, whether there would not be every day more occasion for agriculture? And whether this article alone would not employ a world of people? (II. 237)

The putting in hand of desirable public works could and should be used to raise and maintain the level of employment at home:

Whether it would not be of use and ornament, if the towns throughout this kingdom were provided with decent churches, town-houses, work-houses, market-places and paved streets, with some order taken for cleanliness? (II. 248)

Elsewhere in *The Querist*, and in his *Essay* of 1721, Berkeley proposes, for the central governments of both Britain and Ireland, large schemes of public expenditure, which might, or might not, be met out of taxation, and which would make up, or if necessary more than make up, for the cutting down of private luxury expenditure at home. He mentions education, learned academies, public buildings ('adorning them with paintings and statues'), and, exactly like Petty, 'triumphal arches, columns, statues, inscriptions, and the like monuments of public services'.<sup>1</sup> However, unlike Petty and Steuart, Berkeley does not attempt to analyse how the government can affect the level of aggregate activity by what are now called budget deficits or surpluses. Perhaps this is partly because he laid his main emphasis on monetary policy.

(c) *Monetary institutions and policy*: Berkeley's main attention and emphasis are not on his labour and fiscal policies, important though these are to his programme. His main, central, proposals are those for a national bank and a paper currency. Similarly, his remarks on the nature and functions of money, with which he supports his central

<sup>1</sup> cf. Berkeley, *Essay*, 1721, in Fraser, *Works*, vol. 3, p. 205. Sir W. Petty, also concerned with chronic unemployment, favoured government spending on 'entertainments, magnificent shews, triumphal arches' since this government expenditure is 'a refunding the said money to the tradesmen who work upon these things' (See *Economic Writings*, ed. Hull, vol. I, p. 33.)

proposals, are analytically the most interesting contribution of *The Querist*. Both Berkeley's practical proposals and his analysis are very similar to those of John Law, and may be said to follow the school of thought which Heckscher describes as 'Paper-Money Mercantilism'.<sup>1</sup>

The principal aim of monetary institutions and policy must be to maintain a high level of economic activity and above all to prevent the paradox of poverty and unemployed resources existing side by side. In his *Essay* of 1721 Berkeley had written: 'Money is so far useful to the public as it promoteth industry, and credit having the same effect is of the same value with money'. *The Querist* asks:

Whether money be not only so far useful, as it stirreth up industry, enabling men mutually to participate the fruits of each other's labour? (I. 5)

Whether money circulating be not the life of industry; and whether the want thereof doth not render a state gouty and inactive? (III. 8)

Whether all regulations of coin should not be made, with a view to encourage industry, and a circulation of commerce, throughout the kingdom? (III. 140)

Whether facilitating and quickening the circulation of power to supply wants be not the promoting of wealth and industry among the lower people? (III. 186)

The function of money is to convert a lively 'natural' or 'real' demand into a *monetary* demand adequate to give that high level of employment which is morally and economically desirable. Money has been described as 'coined liberty'; Berkeley would have described it as 'coined power' and insisted that this power should be rightly distributed and circulated:

Whether the real end and aim of men be not power? And whether he who could have every thing else at his wish or will, would value money? (I. 7)

Whether the public aim in every well-governed State be not, that each member, according to his just pretensions and industry, should have power? (I. 8)

No one has ever exposed more repeatedly and fundamentally, even to the point of exaggeration, the error (attributed since Adam Smith to a somewhat indiscriminate body of his predecessors known as 'Mercantilists') that the precious metals are, in some sense, the

<sup>1</sup> On what he calls 'Paper Money Mercantilism', see Heckscher, *Mercantilism*, vol. 2, pp. 231 ff.



ultimate form of wealth, the accumulation of which should be the ultimate end of all economic policy. For example :

What makes a wealthy people ? And whether mines of gold and silver are capable of doing this ? (I. 31)

Whether there be any virtue in gold or silver, other than as they set people at work, or create industry ? (I. 32)

In spite of the strong prejudices attaching to gold and silver, money is essentially a 'ticket', and, as things are, through the reliance on an uncertain supply of the precious metals, tickets of convenient denominations are simply not available. Hence the circulation of payments, which it is the essential function of these tickets to facilitate, is slowed down :

Whether the prejudices about gold and silver are not strong, but whether they are not still prejudices ? (III. 88)

Whether it doth not much import to have a right conception of money ?

And whether its true and just idea be not that of a ticket, entitling to power and fitted to record and transfer such power ? (III. 89)

Money being a ticket, which entitles to power and records the title, whether such power avails otherwise than as it is exerted into act ? (III. 176)

Whether business at fairs and markets is not often at a stand, and often hindered, even though the seller hath his commodities at hand, and the purchaser his gold, yet for want of change ? (III. 179)

Whether beside that value of money which is rated by weight, there be not also another value consisting in its aptness to circulate ? (III. 180)

As wealth is really power, and coin a ticket conveying power, whether those tickets which are the fittest for that use, ought not to be preferred ? (III. 181)

Such being the essential functions of money, it is clear that a supply of paper money can be much more conveniently regulated than can the supply of the precious metals, as had been shown, Berkeley claimed, in America :

Whether it be not agreed that paper hath, in many respects, the advantage above coin, as being of more dispatch in payments, more easily transferred, preserved, and recovered when lost ? (I. 207)

Whether there are not to be seen in America fair towns, wherein the people are well lodged, fed, and clothed, without a beggar in their streets, although there be not one grain of gold or silver current among them ? (I. 284)

Whether paper doth not by its stamp and signature acquire a local value, and become as precious and scarce as gold ? (III. 87)

Paper money is for Berkeley the highest stage in the development of exchange economies, and he sees no important distinction between money and credit (as he had already indicated in his *Essay* of 1721) :

Whether in the rude original of society, the first step was not the exchanging of commodities, the next a substituting of metals by weight as the common medium of circulation, after this the making use of coin, lastly a further refinement by the use of paper with proper marks and signatures ? And whether this, as it is the last, so it be not the greatest improvement ? (III. 100)

Whether all circulation be not alike a circulation of credit, whatsoever *medium* (metal or paper) is employed, and whether gold be any more than credit for so much power ? (III. 10)

To issue and regulate the paper currency a National Bank should be set up. Such banks, Berkeley claimed, were already operating successfully in Venice, Amsterdam, and Hamburg, and one had in fact already been under discussion for Ireland :

Whether all things considered, a national bank be not the more practicable, sure and speedy method to mend our affairs, and cause industry to flourish among us ? (II. 129)

Whether a national bank would not at once secure our properties, put an end to usury, facilitate commerce, supply the want of coin, and produce ready payments in all parts of the kingdom ? (II. 12)

It was vital that the National Bank should be publicly owned and controlled. The power to issue and regulate the supply of money was much too important to be left in private hands, Berkeley considered :

Whether a bank in private hands might not even overturn a government ? And whether this was not the case of the bank of St. George in Genoa ? (I. 214)

Whether by a *national bank*, be not properly understood a bank, not only established by public authority as the Bank of England, but a bank in the hands of the public, wherein there are no shares : whereof the public alone is proprietor, and reaps all the benefit ? (I. 222)

Berkeley went into much detail in his proposals for a National Bank.<sup>1</sup>

<sup>1</sup> 'Letter on the Project of a National Bank,' *Works*, ed. Luce and Jessop, London and Edinburgh, vol. IV, 1951, pp. 185 sqq. His main points were that there should be no private shareholders, and that the directors should not themselves be members of the government or parliament, but should be appointed and periodically inspected by a committee, composed of members of the government and parliament, which would regularly be changing its membership.

But he insisted that any scheme must be treated as experimental and that his own proposals admitted of many variations. He was well aware of the crucial importance of wooing public confidence discretely and cautiously :

Whether there should not be great discretion in the uttering of bank notes, and whether the attempting to do things *per saltum* be not often the way to undo them ? (II. 138)

Whether the main art be not by slow degrees and cautious measures to reconcile the bank to the public, to wind it insensibly into the affections of men, and interweave it with the constitution ? (II. 139)

Berkeley, also, could hardly have been unaware of the dangers of inflation and speculation, of which there had, comparatively recently, been such monstrous examples in London and Paris. Berkeley attributed these disasters to undisciplined private appetites which a publicly regulated monetary authority should be able to control. In any case, it was a chronically depressed economy that he was most concerned with :

Whether we may not easily prevent the ill effects of such a bank, as Mr. Law proposed for Scotland, which was faulty in not limiting the quantum of bills, and permitting all persons to take out what bills they pleased, upon the mortgage of lands, whence, by a glut of paper, the price of things must rise ? (I. 25)

Whether the public aim ought not to be that men's industry should supply their present wants, and the overplus be converted into a stock of power ? (II. 120)

Whether money, more than is expedient for these purposes, be not upon the whole hurtful rather than beneficial to a state ? (II. 122)

Whether therefore bank-bills should at any time be multiplied, but as trade and business were also multiplied ? (II. 124)

The supreme aim of the monetary authority must be a high level of production and employment :

Whether the promoting of industry should not be always in view, as the true and sole end, the rule and measure of a National Bank ? And whether all deviations from that object should not be carefully avoided ?

Finally, a right understanding of the nature and functions of money, followed by appropriate government action could do very much to raise the standard of living of the Irish people :

Whether that which employs and exerts the force of a community deserves not to be well considered, and well understood ? (III. 317)



Whether the immediate moves, the blood and spirits, be not money, paper or metal? (III. 318)

Whose fault is it if poor Ireland still continues poor? (III. 324)

## 4

There does not appear to have been any immediate practical success attributable to *The Querist*, though according to Fraser 'there was an appreciable amendment in the circumstances of Ireland towards the middle of the last (eighteenth) century' which he somewhat vaguely connects with Berkeley's work.<sup>1</sup> A responsible national banking institution was eventually set up in 1783. But the body of 'employment' analysis and policy, finely represented by *The Querist* (though in its main outlines, if not in terminology, the same as what became accepted by the majority of economists almost exactly two centuries later) foundered, in its own day, on two main difficulties. First, there was not the statistical information, or the government administration, to work the analytically sound policies. Secondly, the 'perverse' supply curve of labour was a persistent obstacle, so that the earlier optimistic proposals—like Berkeley's—for guaranteeing the right to work, eventually petered out, later on in the century, in the negative disciplines of the workhouse (originally often intended as a form of public works, as by Berkeley himself).<sup>2</sup>

But even if *The Querist* was no more than normally successful in 'making his countrymen think' (and act) successfully, what of its longer-term contribution in helping subsequent economists to think? Here the success of *The Querist* has certainly been quite undeservedly patchy and slight. Adam Smith presumably must have known Berkeley's pamphlet, but does not show any sign of the influence of some of its most important arguments. Subsequent economists of the classical period could hardly find much of value in *The Querist* if they took at all seriously the Mill-Say analysis of markets, or based their study of a monetary economy on such a proposition as Adam Smith's that 'money can serve no other purpose besides purchasing goods', and 'therefore, necessarily runs after goods', and were thence led to assume that the analysis of a barter economy could be applied without essential modification to a monetary economy. Berkeley would have considered it much rather misleading than helpful to

<sup>1</sup> Cf. Fraser, op. cit., vol. 4, p. 243

<sup>2</sup> Cf. Furniss, op. cit. ch. 5, 'The Enforcement of the Duty to Labor'.

## BERKELEY'S 'QUERIST'

proclaim with Smith that 'we trust with perfect security that the freedom of trade, without any attention of government, will always supply us . . . with all the gold and silver we can afford to purchase or to employ, either in circulating our commodities, or in other uses'. But it was such assumptions as these of the 'Smithian revolution', driven dogmatically home by James Mill and Ricardo, which coloured, at least to a dangerously significant extent, a majority of the more orthodox and influential nineteenth-century writings.

It is hardly surprising that, when Maria Edgeworth commended *The Querist* to Ricardo in the warmest terms, she got no reply on the subject from that quarter.<sup>1</sup> *The Querist* was reprinted in 1829, but the only notice taken of it seems to have come from Sir James Mackintosh, the philosophical opponent of James Mill, who remarked: 'Perhaps *The Querist* contains more hints, than original, still [1829] unapplied in legislation and political economy, than are to be found in any equal space'.<sup>2</sup>

In 1871 Fraser's great edition of Berkeley appeared, and it was reviewed by John Stuart Mill in what must have been one of his last essays.<sup>3</sup> Mill's remarks on *The Querist* are too brief for any sure interpretation. But they convey at least a strong unconfirmed suggestion that Mill was still not clear of the typical orthodox misunderstandings of seventeenth- and eighteenth-century economic thought, exemplified in his wholesale youthful condemnations (1829) of all his predecessors of 'the last two centuries' (presumably excluding Adam Smith).<sup>4</sup> Mill praises Berkeley for considering 'luxurious expenditure a detriment', and for his perception 'that money is not in itself wealth, but a set of counters'—which perception, Mill very questionably claims, places Berkeley much in advance of his age. But Mill's praise of Berkeley for his condemnation of luxury requires some essential qualifications which it is doubtful whether Mill would have accepted.

One might perhaps assume from Mill's praise, that Berkeley, with his strong disapproval of luxury, was much nearer as an economist to Adam Smith, with his unconditional eulogy of 'parsimony', than he was to Mandeville, who cynically defended the public beneficence

<sup>1</sup> See Ricardo's *Works*, ed. Sraffa, vol. 9, p. 231

<sup>2</sup> J. Mackintosh, *Dissertation on the Progress of Ethical Philosophy*, 1829, p. 211

<sup>3</sup> See J. S. Mill, *Dissertations and Discussions*, vol. 4

<sup>4</sup> See J. S. Mill, *Essays on Some Unsettled Questions of Political Economy*, pp. 47-48, and p. 73

of 'vicious' luxury expenditure. To make such an assumption would, however, be to make a complete mis-classification and misunderstanding of Berkeley's economics. It is, of course, true that *purely morally*—and Berkeley did not separate his moral and his economic doctrines—he considered luxury expenditure 'a detriment' and was a severe critic of Mandeville. But if one separates out Berkeley's economic analysis there can be no question that here he was on the same side of the fence as Mandeville, along with Petty, Barbon, Steuart and countless lesser writers; and that he would have rejected the economic analysis of saving and investment (or that saving *is* investment) on which Adam Smith based his eulogy of parsimony (however much *morally* Berkeley would have approved). We entirely agree with Professor Johnston that Berkeley, on the economic issue 'shared Mandeville's opinion' that there was an important problem in maintaining a high level of aggregate demand.<sup>1</sup> Berkeley condemned Mandeville's cynical reliance on luxury expenditure in the midst of mass poverty, but laid as much emphasis as he could on a high level of government expenditure (on roads, bridges, drainage, public buildings and works of art) and on raising the standard of living of the poor. Berkeley emphatically did not believe that the level of aggregate effective demand could be left to settle itself, and that incomes not spent in one way would inevitably get spent in another, and therefore that 'parsimony', private and public, is unconditionally beneficial.

It is worth lingering on this point because to the period of Berkeley's *Querist* (1735-50), and to the morally similar, but economically deeply contrasting, criticisms of Mandeville by two great philosophers at that time, may well be traced the parting of the ways for more than a century and a half of British economic thought. Berkeley and Hutcheson (Adam Smith's revered teacher in Glasgow) were both morally incensed by Mandeville.<sup>2</sup> Berkeley's point of view we

<sup>1</sup> J. Johnston, 'The Monetary Theories of Berkeley,' *Economic History*, 1938, pp. 21-24.

<sup>2</sup> In the inspiring penultimate chapter of the *General Theory*, where Keynes discusses Mandeville and other forerunners of his ideas, he puts forward a very odd generalisation to the effect that after Mandeville the doctrine *critical of* 'the utmost thrift and economy both by the individual and the state . . . did not reappear in respectable circles for another century, until in the later phase of Malthus the notion of the insufficiency of effective demand takes a definite place as a scientific explanation of unemployment'. (*General Theory*, p. 362.) This is simply to wipe out the best part of a century of British and French economic thought. Malthus on this



## BERKELEY'S 'QUERIST'

have described as the most complete moral opposition, but with every implication of agreement on pure economic analysis or the relevant economic assumptions. Hutcheson's criticism of Mandeville is based less on moral and political grounds and more on a fundamentally contrasting *economic* analysis, as far as this goes. For Hutcheson, income not spent in one way will be spent in another, and if not wasted in luxury will be devoted to prudent useful purposes. Until everyone in the world has attained to the necessities of life, aggregate demand can be, and (it seems inevitably to be assumed) *will* be, at an adequate level, or even a 'maximum' level, without any 'vicious' luxury expenditure.<sup>1</sup> Hutcheson's simple sentences,

subject was a not-always-consistent conservative, trying to cling on to some of the seventeenth- and eighteenth-century ideas against the ardent revolutionary dogmatists of the New Political Economy. Moreover, to say, as Keynes did (*Eugenics Review*, 1937, vol. I, p. 16) that 'Malthus first told us' about 'the devil of unemployment' is historically unfounded. Unemployment had been a central preoccupation of British and French economists for at least 150 years before Malthus, as is clear from the writings of such leaders as Petty, Boisguillebert, Berkeley, and Steuart, and from Furniss's book which summarises much of the rank-and-file opinion—a book written in 1920 which it is now most illuminating to read through Keynesian spectacles. Though calling attention to what seem to be some important gaps in Keynes' brief pioneer sketch of this phase in the history of economic thought, we would reject entirely the treatment of Keynes' chapter on this subject by some of his most enthusiastic disciples. Mr. Harrod, for example, has written (*Life of J. M. Keynes*, London, 1951, p. 460): 'In Keynes' handling of the mercantilists, he appears to me to have seized on isolated passages to find wisdom that was not really there. "Roy strongly objects to Chapter 26" he wrote to Mrs. Robinson, "as a tendentious attempt to glorify imbeciles."' It is not quite clear who is calling whom an imbecile. But if, by chance, this term was really being applied to Petty, Mandeville, Berkeley, and Steuart, it would certainly only be in the tradition of the classical treatment of their predecessors, as exemplified, in particular, by J. S. Mill and Fawcett.

<sup>1</sup> Cf. Hutcheson, *Remarks upon the Fable of the Bees* (Glasgow, 1750, first published c. 1727): 'Unless therefore all mankind are fully provided not only with all necessities, but all innocent conveniences and pleasures of life, it is still possible without any vice, by an honest care of families, relations, or some worthy persons in distress, to make the greatest consumption' (p. 63). Hutcheson then seizes upon one of the most far-fetched examples of Mandeville, his argument that robbers are good for trade as they stimulate production by locksmiths: 'Who needs be surprised that luxury or pride are made necessary to public good, when even theft and robbery are supposed by the same author to be subservient to it, by employing locksmiths? . . . Were there no occasion for locks, had all children and servants discretion never to go into chambers unseasonably, this would make no diminution of manufactures; the money saved to the housekeeper would afford either better dress, or other conveniences

in themselves apparently harmlessly general, led on, in the work of his great pupil, to the new analysis of saving and investing, the assumption that income not spent in one way is always spent in another, since no one wants to hold money for its own sake, and so, in due course, with the aid of Tucker and Turgot, and later J. B. Say, and J. Mill, to the 'classical' analysis of markets, the doctrine of 'the impossibility of general overproduction', to the Ricardo-Treasury view that public works will not diminish unemployment, and to the orthodox dismissal of all 'under-consumptionist' arguments.

At a vital juncture in the second quarter of the eighteenth century Hutcheson pointed to the high-road of nineteenth-century orthodoxy, while Berkeley, like most of his contemporaries and predecessors, kept along the low road, which, for almost a century and a half from Adam Smith to Keynes, was in Britain followed only by a minority of unorthodox 'cranks'. The roads have now joined up again, and fortunately we do not have to enquire here how drastically those following the orthodox high-road had to change their direction, how

*to a family, which would equally support artificers: even smiths themselves might have equal employment. Unless all men be already so well provided with all sorts of convenient utensils, or furniture, that nothing can be added, a necessity or constant usefulness of robbers can never be pretended, any more than the public advantages of shipwrecks and fires, which are not a little admired by the author of the fable'* (pp. 64-65, italics supplied). It may well be very reasonable, in some contexts, to assume that all income-receivers will spend their money on one thing (household 'utensils') if they don't have to spend it on another (padlocks), but the development of Hutcheson's notion into a universal axiom had serious consequences in some lines of economic investigation. A hundred and fifty years later Alfred and Mary Marshall, with the same degree of significance, advanced the same assumption that income and resources not used in one way will be used in another: 'It is not good for trade to have dresses made of material which wears out quickly. For if people did not spend their means on buying new dresses they would spend them on giving employment to labour in some other way.' (*Economics of Industry*, 1879, p. 17.) Keynes went so far as to hold, in 1936, that 'contemporary thought is still deeply steeped in the notion that if people do not spend their money in one way they will spend it in another.' (*General Theory etc.*, p. 20.) We suggest that the decisive injection of this simplifactory but questionable assumption into the orthodox body of economic thought comes from Adam Smith's teacher Hutcheson.

It may be noted that though Adam Smith built wholeheartedly on Hutcheson's assumptions, the perfectly-balanced Hume, like Berkeley, accepted Mandeville's economic argument. See his essay on *Refinement in Arts*, which puts him in the opposite camp to Hutcheson and Smith. On how very much Smith's economic theory owed to Hutcheson see G. Bryson, *Man and Society*, etc., Princeton, 1945, p. 215.

## BERKELEY'S 'QUERIST'

much (if any) of the orthodox intellectual equipment had to be jettisoned in the change of course, and how much was lost by the long neglect of the sort of ideas of which Berkeley was such a distinguished expositor.

One thing, however, which we suggest might be finally discarded—if it does still survive—is the view of eighteenth-century economic thought (including Berkeley) held by such orthodox authorities as J. S. Mill, Henry Fawcett, Leslie Stephen, and Marshall; and along with their views might go that liberalist-progressivist interpretation of eighteenth-century economic thought according to which the more sweepingly a political economist introduced and generalised the assumption of a satisfactorily self-equilibrating mechanism throughout most of the economic universe, the more advanced, enlightened, and correct he inevitably was.

J. S. Mill, for example, goes on in his review to suggest that if Berkeley had 'followed up his ideas further he might have anticipated the work of Adam Smith' as though this must be the sole, the highest, and the most inevitable criterion of intellectual achievement for economic writers before 1776. Marshall was expressing a similar view of eighteenth-century history when he wrote of Adam Smith: 'Whenever he differs from his predecessors he is more nearly right than they; while there is scarcely any economic truth now known of which he did not get some glimpse.'<sup>1</sup> On the contrary, it seems clear

<sup>1</sup> See Marshall, *Principles*, 8th ed., p. 757. Marshall's predecessor at Cambridge, Henry Fawcett, in generalising about eighteenth-century (and earlier) effective demand theories had not even omitted Adam Smith from his generalisations. In his much-studied *Manual of Political Economy*, 6th edn., 1883, he writes (pp. 472-473): 'All political economists who preceded James Mill and Ricardo, and many who have succeeded them, seem to anticipate a general over-production of commodities as a possible or even probable contingency.' And Fawcett goes on to explain that by over-production he is assuming that all these economists meant *absolute* over-production irrespective of price, i.e. 'a greater quantity of all commodities may be produced than people really want'.

The influential and distinguished Leslie Stephen unfortunately took his political economy straight from Fawcett. It is therefore hardly surprising to find him not even mentioning Berkeley's *Querist* in his *History of English Thought in the Eighteenth Century*. Stephen must have included *The Querist* in that 'incoherent mass of empirical maxims' which 'for the first time . . . was codified into a definite system and elevated to the dignity of a science' by Adam Smith: 'The English economists before the appearance of the *Wealth of Nations*, claimed only to be adepts in the mysteries of commercial accounts. After it, they began to regard themselves as investigators of a new science capable of determining the conditions and the



to us that a large part of Berkeley's achievement is his lucid practical statement of an economic truth of which Adam Smith showed strangely little sign of having glimpsed, and which it was a main consequence of Smith's doctrines, and those of his more dogmatic successors, largely to conceal and even to suppress in Britain for a very long time. This particular economic truth is, of course, that an inadequate level of employment and effective demand may be a serious problem which state action by fiscal, monetary, and other policies can and ought to deal with.<sup>1</sup> Unless one believes that Adam Smith was 'more nearly right' on this subject than, say, Lord Keynes, or the British White Paper on *Employment Policy* of 1944, then one cannot hold that Smith was more nearly right than Berkeley (or Petty or Steuart). In fact, the main assumptions, analysis, and programme of Berkeley's *Querist* are very closely similar in essential outline to those which Keynes argued for in the inter-war years: that is much more centralised monetary management, public works, and tariffs if necessary to protect the balance of payments, all with the objective of raising the level of employment and productivity above its depressed level. One can readily imagine Berkeley agreeing, for the Ireland and Britain of his day, with Keynes' generalisation about

*limits of human progress.*' (op. cit., vol. 2, p. 283; my italics.) There is clearly much truth in Stephen's generalisations, but unfortunately 'codification' meant over-simplification, and the omission of much that had been important to Smith's predecessors in the eighteenth century, and was to be so again to his successors in the twentieth century.

<sup>1</sup> Professor Viner holds that in Bentham's time 'it was too early for proposals to stabilise employment through monetary or fiscal measures' (*American Economic Review*, 1949, p. 362). As we see it, it was definitely not too early for such proposals in 1662, for example, when Petty wrote his *Treatise*, or in 1735 when Berkeley wrote his *Querist*, or in 1767 when Steuart wrote his *Principles*—to take three leading examples only. On the contrary, by Bentham's time it was *too late* for such proposals, which had been superseded by Smith's analysis of saving, investing and the holding of money, which was to be developed by some of Bentham's own closest associates into the classical theory of markets and the Ricardo-Treasury 'view' on public works. Further, we are not quite clear how the existence of Berkeley's *Querist* is compatible with the generalisation that 'there was not until the very last moments of the century either a single major political debate which turned on the economic conditions of the poor or a single major writer who had important suggestions as to how to improve them, with the sole exception of Adam Smith's plea for freedom of trade'. (Viner, op. cit., p. 361, my italics.) It may be that Berkeley is not to be classified as a 'major writer' simply because he was not a forerunner or disciple of Adam Smith.

the British inter-war economy, that 'the outstanding faults of the economic society in which we live are its failure to provide for full employment and its arbitrary and inequitable distribution of wealth and incomes' (*General Theory, etc.*, p. 372). Of course, one may take the view that the Keynesian theory was 'largely an emotional and hysterical reaction to crisis conditions, a "depression psychosis" . . . based more on temperament and bad temper than on sober appraisal and objective judgement.'<sup>1</sup> Of course, an historian of thought who takes such a view of Keynes' theory is not likely to see much significance in the writings of Berkeley, or Petty, or Mandeville, or Steuart. If it seems that Keynes (and others) had to fight a hard battle of rediscovery this can only have been necessary because of the triumphantly successful intellectual aggression of the over-simplifiers of the early nineteenth century. The line of thought to which Berkeley was such a distinguished contributor—though only one of many in eighteenth-century economic thought—*could* have continued uninterrupted through the nineteenth century, whatever the policies which had or had not been found appropriate from decade to decade. After the 'Keynesian Revolution' there are still many people left who can read Marshall and Walras with comprehension. After the 'Jevonian Revolution' the doctrines on value of J. S. Mill and even Ricardo were still met with wide and orthodox sympathy. But after the 'Smithian Revolution', as dogmatically driven home by J. Mill and Ricardo, the doctrines on effective demand, and on macroeconomic analysis generally, of Petty, Berkeley, Steuart, Boisguillebert, Quesnay, and many lesser writers, passed completely beyond the pale of orthodox comprehension, until finally we have the extraordinary spectacle of the Professor of Political Economy at Cambridge proclaiming unchallenged in a best-selling textbook that all political economists who preceded James Mill and Ricardo anticipated as a probable contingency that more commodities may be produced than people will really want at any price.

It is the force and distinction of *The Querist's* exposition, rather than any great originality or subtlety of analysis, which places it along with the writings of Petty and Steuart, as one of the leading pre-classical statements of macroeconomic analysis and policy, before *The Wealth of Nations* initiated the 'classical' approach based to a considerable extent on the assumption that the main aggregates of

<sup>1</sup> J. Viner, *International Trade and Economic Development*, Oxford, 1953, pp. 9-10

demand and supply tended—apart from periodic frictions—to be, on the whole, satisfactorily self-regulating.

It might perhaps be said of Berkeley, as Sidgwick said of Mill, that his main achievement was to have 'brought a higher degree of philosophical reflection to bear upon the exposition of the common doctrines' (of political economy such as they were in 1735). With Berkeley this 'higher degree of philosophical reflection' worked to heighten his reliance on a practical imprecise common sense, rather than in the direction of a clear-cut formulation and 'codification' (which in the social sciences has usually in fact meant over-simplification, and much more over-simplification than has usually been first realised at the time). Such analytical generalisations as Berkeley uses (on value, or on the quantity theory of money)<sup>1</sup> are thrown out *ad hoc*, the practical problem shaping and determining the 'tools of thought', as economists call them, rather than the other way round.

Was Berkeley a liberal or a socialist? The question is anachronistic and, of course, rather absurd. In his epoch he was one of the greatest (perhaps *the* greatest) of philosophers and Christians on the side of intellectual and religious liberalism. But 'economic liberalism' seems to be a separate and often much narrower concept. Anyhow, it seems difficult to describe as an 'economic liberal' in any sense of this rather unsatisfactory term, one who advocated *some* measure of compulsory labour, sumptuary laws, extensive public works, control of speculation, and the principle of economic planning.<sup>2</sup> On the other hand, though Berkeley was certainly very much of a paternalist, we would completely disagree with a French description of *The Querist* as 'a defence of economic authoritarianism'.<sup>3</sup> For Berkeley was essentially a middle-of-the-road moderate, and a man of empirical and not *a priori* maxims. Writing on the morrow of the South Sea Bubble, and surrounded by the paradox of chronic unemployment and poverty, Berkeley stood for a great deal of control and direction, and

<sup>1</sup> 'Whether the value or price of things, be not a compounded proportion, directly as the demand, and reciprocally as the plenty' (I. 24).

'Whether, *ceteris paribus*, it be not true that the price of things increase, as the quantity of money increaseth, and are diminished as that is diminished' (III. 157).

<sup>2</sup> 'Whether if a man builds a house he doth not in the first place provide a plan which governs his work? And shall the public act without an end, a view, a plan?' (I. 53).

<sup>3</sup> See A. V. Espinas, 'La troisième phase et la dissolution du mercantilisme', *Revue internat. de sociologie*, 1902, 10, 179.



### BERKELEY'S 'QUERIST'

did not consider as within the scope of practical politics the devising and setting up of self-adjusting individualist price-mechanisms. He could hardly have been unreservedly enthusiastic about some of the manifestations of the new economic liberalism of his day, especially in the field of speculation. Nor did he regard economic liberty as an end in itself. Here is a final quotation indicative of his general position: 'Liberty is the greatest human blessing that a virtuous man can possess and is very consistent with the duties of a good subject and a good Christian. But the present age aboundeth with injudicious patrons of liberty. . . . It hath been always observed of weak men, that they know not how to avoid one extreme without running into another.'<sup>1</sup>

<sup>1</sup> Berkeley, *Works*, vol. 3, ed. Fraser, p. 195

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## AN OUTLINE OF BERKELEY'S LIFE

J. O. WISDOM

GEORGE BERKELEY was born in Ireland in the county of Kilkenny on 12th March 1685, probably at Kilcrin.<sup>1</sup> Little is known about his parents. His father, William Berkeley, was English by immediate descent, and was probably born in England but lived in Ireland, where he held a government post of some repute. Berkeley's mother was almost certainly Irish; she was great-aunt to General Wolfe. Swift spoke of Berkeley as a kinsman of Lord Berkeley of Stratton, sometime Lord Lieutenant of Ireland; circumstantial evidence supports this, but documentary proof is lacking. George was the eldest of a family of six sons and one daughter, unless there were others that died in infancy. He appears to have had an excellent record at the famous Kilkenny School, where Swift had been not long before; here began his life-long friendship with Thomas Prior, one of the founders of the Dublin Society. Little is known about his schooldays.

At the age of fifteen, at the dawn of the eighteenth century, he entered Trinity College, Dublin, where he graduated, took his Master of Arts, became a Fellow and lecturer, and was ordained in the Church of Ireland. Intellectually the atmosphere was very much alive; the writings of Locke, Molyneux, Descartes, Hobbes, and Newton were all familiar in the College circle. The British movement of deism was started in 1696 by John Toland, an Irishman; on the opposite side, the Provost of Trinity and the Archbishop of Dublin were famous theologians. In addition to reading the works of these men with great care, Berkeley made a close study of Malebranche.

He composed some slight mathematical pieces, probably as a Fellowship thesis, but most important of all he recorded his philosophical thoughts in two notebooks, and thus has left us what is now known as the *Philosophical Commentaries*, which shows us the amazing freshness of his mind, his early sceptical trend, and the seeds of his

<sup>1</sup> Three different birth places are given by good authorities. Luce has examined the evidence and decided in favour of Kilcrin; A. A. Luce, 'The Purpose and Date of Berkeley's *Commonplace Book*. With an Appendix on Berkeley's Birthplace', *Proc. of the Royal Irish Acad.*, Dublin, 1943, 48, Sec. C, No. 7, pp. 286-289.

## AN OUTLINE OF BERKELEY'S LIFE

mature philosophy. This vitality was manifested and fostered by a society, which he founded, for the discussion of philosophy.

Within two years of becoming a Fellow, Berkeley produced his first great work, which was on the psychology of visual perception. Though this was a scientific work on its own account, it also served as a prelude to his classic work on philosophy, which appeared the next year in 1710: *The Principles of Human Knowledge*. Three sermons on Passive Obedience, delivered in the College Chapel, seemed likely to injure his career, because, through an understandable misinterpretation, they led to his being taken to be a Jacobite. To counter this, he fused them into a tract, but the publication of it did not entirely dispel the unfortunate rumour. Still, unlike Hume and others, he was never made to suffer for his ideas.

Berkeley then put his philosophy in the form of three dialogues, perhaps the most famous in the English language. He left Dublin for London in January 1713, a few months before the *Three Dialogues* was published, and there he met many of the illustrious men of letters of the early eighteenth century. He and the dialogues soon gained an excellent reputation. Swift presented him to Lord Berkeley of Stratton, according to report, with the words: 'My Lord, here is a young gentleman of your family. I can assure your Lordship it is a much greater honour to you to be related to him, than to him to be related to you.'<sup>1</sup> He met Dr Clarke, a metaphysician with whom he regularly discussed questions of philosophy at St James's Palace for the edification of Princess Caroline. And he seems to have visited some of the clubs frequented by deists and freethinkers, which doubtless furnished him with copy, not only for his *Alciphron* of later years, but also for his essays which were soon to appear in the *Guardian*.

A few months later he set out on his first continental tour, as chaplain to Lord Peterborough, who was going to Sicily as Ambassador Extraordinary on the coronation of the new king. Ten months abroad showed him a good deal of France and Italy, and seems to have whetted his appetite for more, for after spending two years in London, we find him again in Italy in 1716, this time as travelling tutor to the son of the Bishop of Clogher. It was probably in the intervening period that he composed a treatise on ethics which he lost on his next tour. In the course of his wanderings during the next five years, he kept a journal, which is remarkable for the close attention he gave to

<sup>1</sup> A. C. Fraser, *Life and Letters of George Berkeley*, Oxford, 1871, p. 6n.



detail of all kinds. Before returning he wrote his *De Motu* and published it in London in 1721. It supplements in one or two respects the philosophy of the *Principles*, but it is mainly a treatment of physical concepts in their own right. It is a most important contribution, which, like his mathematics, philosophers have either ignored or underrated.<sup>1</sup> After a year in London Berkeley returned to Dublin and received the degree of Doctor of Divinity.

Berkeley was now much occupied with the task of getting a deanery. As happened more than once in his life, several authorities claimed the power of making an appointment; and now, after being given the Deanery of Dromore by one of these, he found himself engaged in a long and tiresome lawsuit to defend his right to it. This affair seems to have faded out, as he was made Dean of Derry two years later, in 1724. Meanwhile he came in for a legacy in curious circumstances. Swift's Vanessa left her fortune to Berkeley and another, although she was not personally acquainted with Berkeley. As things turned out, her debts swallowed up most of the legacy, but the prospect of the fortune made it possible for Berkeley to think in good earnest about a project that otherwise might have remained a day-dream. He decided to spend the remainder of his days in Bermuda. His scheme was a brilliant and a tragic one. He planned to found a College for the education and conversion of American Indians; he pursued this with all his might as he never had anything before, and never would again. By sheer force of character he obtained large private subscriptions, a vote from Parliament, a Royal Charter, and the help of willing academic colleagues. Doubtless the influence of the philosophical Princess Caroline proved of value: she liked to have Berkeley attend her philosophical circle. There was a long delay in the payment of the State grant, and Berkeley thought it best to go across the ocean and await the money there.

Just a month before he sailed for America in 1728, at the age of forty-three, he had married Miss Anne Forster, daughter of the Speaker of the Irish House of Commons, reputed to be a religious mystic and to have a taste for books. It has puzzled the Berkeley authorities that so many of his letters from London about this time to his friend Tom Prior should have been about the renting of a

<sup>1</sup> Elsewhere I have mentioned a long line of thinkers whose view of physical concepts was anticipated by Berkeley. See J. O. Wisdom, 'The Descriptive Interpretation of Science', *Proc. Arist. Soc.*, London, 1942-3, 43; *Foundations of Inference in Natural Science*, London, 1952, Ch. IX.

## AN OUTLINE OF BERKELEY'S LIFE

house about which he urged the utmost secrecy: why should he have wished to travel to Dublin incognito? Surely Miss Forster supplies the answer; he would not have been willing to spend his time in other company, and possibly he did not wish his attentions to be subject to the public gaze. Be that as it may, we now know from a letter discovered not long ago that he did go to Dublin, and that he was there two months before his marriage took place.<sup>1</sup>

In America Berkeley settled near Newport, Rhode Island, and built himself a house named Whitehall. Here he lived in some seclusion, but he moved about enough to be well known to the residents, and was much liked, both for his charm of character and for his religious tolerance. He made the acquaintance of the American thinker, Samuel Johnson, who came to accept the Berkeleian philosophy almost without reservation. In these surroundings he wrote *Alciphron*, a long work of dialogues attacking the deists, which was published in London almost immediately on his return to Europe.

He stayed nearly three years in Rhode Island, waiting for money which never came. The Government did not repudiate the grant, but Walpole let it be known that the money would not be paid, and Berkeley was forced to return home. This disaster had a grave effect upon his health. In addition, he had family losses. Some of his children died young, and his wife had miscarriages.

All through Berkeley's life, his great friend, Sir John Percival of County Cork, who later became the first Earl of Egmont, spared no pains on his behalf. Percival was a realistic and high-minded man, who pursued practical ideas with energy. His friendship with Berkeley began in 1709 when the philosopher was still at Trinity, and their correspondence continued till Percival's death. The easy dignity of their letters can be savoured only from the letters themselves; fortunately all that are known are published. Percival now did his best to have Berkeley appointed to a position worthy of him, to reinstate his reputation after the American venture. This was no easy matter. Berkeley was maligned as a madman on account of his Bermuda scheme; Peers, Bishops, Archbishops, and even the King and Queen were involved in the wrangle about an appointment for him. Eventually, chiefly owing to the influence of the Queen, he was consecrated as Bishop of Cloyne in 1734.

<sup>1</sup> A. A. Luce, 'Berkeley's Bermuda Project . . .', *Proc. Royal Irish Acad.*, Dublin, 1934, 42, Sec. C, No. 6, p. 99

## J. O. WISDOM

About this time Berkeley's writings took the form of tracts. He produced a further work on vision. This was written to elucidate and justify his previous views and to reinforce his work against the deists ; it is important as a framework for his early psychology, but it contained none of the scientific quality of that great early work. Then he was soon involved in the mathematical controversy about fluxions, in which he was right and many of the mathematicians of the day were wrong ; he showed that there was a fundamental flaw in Newton's method. The importance of this great negative achievement has been inadequately recognised.

Settled in his diocese in County Cork, Berkeley was not long in discovering the poverty and sickness around him. The next year, at the age of fifty, he brought out his first economic pamphlet. His proposals are notable for their Keynesian approach and practical nature, but though widely read they were not adopted. He followed with other pamphlets on the same subject.

Several years later Berkeley produced the most extraordinary work ever written by a modern philosopher, *Siris*, an investigation of the medical and divine properties of tar-water. He was in search of a panacea, partly because of the conditions of health in his neighbourhood, but also because certain troubles of his own, which had dogged him for a number of years, had now come to a head. Fantastic though the ideas of this work are, it should not be thought that he had become senile.

In the last years of his life he lost his favourite son. And his two oldest and best friends, Prior and Percival, died within a short time of each other. He declined the honour of being considered for the position of Vice-Chancellor of Dublin University. He went to Oxford and took a house in Holywell Street. He was by now very weak, and he lived to enjoy the academic seclusion for only a few months. He died peacefully in 1755, when he was almost sixty-eight. He was buried in the chapel of Christ Church. So died scholar, psychologist, philosopher, man of letters, courtier, traveller, educational missionary, Christian apologist, bishop, mathematician, economist, and amateur doctor.

It remains to say a few words about Berkeley's personality. Everything that comes down to us, including a fair-sized correspondence, reveals him as a man of unusual charm, possessing the great majority of those character-traits we admire and almost none of



## AN OUTLINE OF BERKELEY'S LIFE

those we do not. 'So much understanding', exclaimed Bishop Atterbury, 'so much knowledge, so much innocence, and such humility, I did not think had been the portion of any but angels till I saw this gentleman.'<sup>1</sup> It is not uncommon to feel that a person with well-nigh perfection of character would be insufferable. Berkeley, on the contrary, was a man much valued by numerous friends, and was one of the most attractive personalities among the great philosophers. Percival records sentiments about him similar to Atterbury's. His wife, a lady of considerable depth of character, has a special right to be heard; writing to her son she said:

In childhood you were instructed by your father—he, though old and sickly, performed the constant, tedious task himself, and would not trust it to another's care. You were his business and his pleasure . . . and when he spoke directly of religion (which was seldom) he did it in so masterly a manner, that it made a deep and lasting impression. You never heard him give his tongue the liberty of speaking evil. Never did he reveal the fault or secret of a friend . . . as he saw no one his superior, or perhaps his equal, how could he envy any one? . . . He was also pure in heart and speech; no wit could season any kind of dirt to him, not even Swift's.<sup>2</sup>

Berkeley was a powerful advocate of marriage, and wrote eloquently of the joys of family life. There are amusing fragments of letters in which he referred to a friend, who was about to marry again, as being 'made happy the third time (*O terque quaterque beatus*)'; he wished his bachelor correspondent 'would once . . . dare to do what he does so often'.<sup>3</sup> When Berkeley's son, William, died, he wrote a letter to Bishop Benson that could hardly have displayed his attachment and grief more strongly.<sup>4</sup>

The old idea of Berkeley as an impractical idealist is, of course, exploded; his actions and his practical ideas were obviously thoroughly realistic. One interesting little detail that is hardly known is the following. After the rebellion of '45, he armed the neighbourhood of Cloyne as best he could, and wrote two letters to the press, urging the virtues of military excellence together with a concrete proposal for forming a militia:

The modern discipline delighteth much in parade. This is a clog upon our levies and recruits, depriving the public of the service of many a

<sup>1</sup> Fraser, *op. cit.*, p. 59

<sup>3</sup> *Ibid.*, p. 268

<sup>2</sup> *Ibid.*, pp. 357-358

<sup>4</sup> *Ibid.*, p. 325

## J. O. WISDOM

stout active fellow, who falls short of the present standard. And yet such a one hath his advantages. At a distance he is a less mark, and in close fight less embarrassed, more nimble either to avoid or give a blow, he is fitter for dispatch in marches and pursuits, in passing through bad roads, in clambering over rocks and mountains, and scaling of walls, he is a less burthen on a horse or carriage.<sup>1</sup>

This and more in the same vein could hardly be more apt in connection with the Home Guard formed in Britain during World War II.

For a full account of the philosopher's life, the reader must turn to the correspondence and Luce's standard biography,<sup>2</sup> which supersedes Fraser's.<sup>3</sup> Prefixed to Rand's<sup>4</sup> edition of the Percival correspondence is an excellent essay on Berkeley's life, but its purpose is restricted to providing the background to the letters. Far and away the best general essay on his life was Balfour's, last printed as an introduction to Sampson's edition of Berkeley's works.<sup>5</sup> Even the earliest of these, Fraser's, is remarkably accurate, though it contains some errors; the newest details exist only in Luce's papers and *Life*.

### BERKELEY'S LIFE : CHRONOLOGICAL OUTLINE

(Only major works are included here : for bibliography see T. E. Jessop, 'A Bibliography of George Berkeley', Oxford, 1934)

- 1685 March 12. Born near Kilkenny
- 1696 Entered Kilkenny College
- 1700 Entered Trinity College, Dublin
- 1707 Elected Junior Fellow
- 1707-8 Wrote *Philosophical Commentaries*
- 1709 Published *An Essay towards a New Theory of Vision*
- 1710 Published *A Treatise concerning the Principles of Human Knowledge*
- 1713 January. Left Dublin for London
- 1713 May. Published *Three Dialogues between Hylas and Philonous*
- 1713 October. Accompanied the Earl of Peterborough to Italy as chaplain

<sup>1</sup> A. A. Luce, 'More Unpublished Berkeley Letters and New Berkeleiana', *Hermathena*, Dublin, 1933, 23, 46

<sup>2</sup> A. A. Luce, *The Life of George Berkeley*, London and Edinburgh, 1949

<sup>3</sup> Fraser, op. cit.

<sup>4</sup> Benjamin Rand, *Berkeley and Percival*, Cambridge, 1914

<sup>5</sup> A. J. Balfour, 'Biographical Introduction', in *Works*, ed. by Sampson, London, 1897, Vol. I

## AN OUTLINE OF BERKELEY'S LIFE

- 1714 August. Returned to England
- 1715 October 13. Malebranche died
- 1716 Autumn. Went abroad as travelling tutor to St George Ashe ; went as far as Sicily. Co-opted Senior Fellow in his absence
- 1730 Autumn. Returned to London
- 1721, early. Published *De Motu*
- 1721 September. Returned to Dublin. D.D. conferred during following term
- 1722 Presented by the Crown to the Deanery of Dromore ; this contested by the Bishop
- 1722, about May. Conceived project of founding a College in Bermuda
- 1722 December. Visited London ; nearly wrecked on a thirty-six hour crossing to Holyhead
- 1723 March. Returned to Dublin
- 1723 June. Legatee of Esther Vanhomrigh
- 1724 May. Resigned from T.C.D. to become Dean of Derry
- 1724 September. Left for London to raise funds for Bermuda project and get Royal Charter
- 1725 March 15. Report on Bermuda project by Law Officers (Attorney-General and Solicitor-General)
- 1726 May 11. House of Commons voted a grant for St Paul's College, Bermuda
- 1728 August. Married Anne Forster
- 1728 September (end of first week). Sailed for America. Arrived Newport 23rd January 1729. Embarked for return 21st September 1731
- 1729 June. Birth of son, Henry
- 1731 October 30. Arrived in England
- 1732 Published *Alciphron*. Nominated Dean of Down ; not appointed
- 1733 Published *The Theory of Vision Vindicated and Explained*
- 1733 September 28. Birth of son, George
- 1734 May. Back to Ireland as Bishop of Cloyne, where he remained for eighteen years
- 1734 Published *The Analyst*
- 1735 Took part in mathematical controversy
- 1735-7 Published *The Querist*
- 1736 December 10. Birth of son, William
- 1737 Made his only speech in the Irish House of Lords
- 1738 October 15. Baptism of daughter, Julia
- 1739 Famine and fever at Cloyne
- 1741 Declined offer of nomination for Vice-Chancellorship of Dublin University
- 1744 Published *Siris*



## J. O. WISDOM

- 1745 Declined offer of Bishopric of Clogher
- 1748 May 1. Death of Percival
- 1751 March 3. Death of son, William
- 1751 October 21. Death of Tom Prior
- 1752 August. Went on prolonged visit to Oxford
- 1753 January 14. Died. Buried at Christ Church, Oxford

### WORKS ON SCIENCE AND PHILOSOPHY OF SCIENCE

- 1707 *Arithmetica absque Algebra aut Euclide Demonstrata. Cui Accesserunt, Cogitata Nonnulla de Radicibus Surdis, de Aestu Aeris, de Ludo Algebraico, &c.* (Eng. trans. in *Works*, ed. by George Sampson, London, 1897, vol. 1)
- 1709 *An Essay towards a New Theory of Vision*
- 1721 *De Motu; sive de Motus Principio & Natura, et de Causa Communicationis Motuum.* (Eng. trans. in *Works*, ed. by A. A. Luce and T. E. Jessop, London and Edinburgh, 1951, vol. 4)
- 1733 *The Theory of Vision or Visual Language, Shewing the Immediate Presence and Providence of a Diety, Vindicated and Explained*
- 1734 *The Analyst; or, a Discourse Addressed to an Infidel Mathematician. Wherein it is explained whether the object, principles, and inferences of the modern analysis are more distinctly conceived, or more evidently deduced, than religious mysteries and points of faith*
- 1735 *A Defence of Free-Thinking in Mathematics. In answer to a pamphlet of Philalethes Cantabrigiensis, intituled, Geometry No Friend to Infidelity, or a Defence of Sir Isaac Newton, and the British Mathematicians. Also an appendix concerning Mr Walton's Vindication of the Principle of Fluxions against the Objections Contained in The Analyst. Wherein it is attempted to put this controversy in such a light as that every reader may be able to judge thereof*
- 1735 *Reasons for Not Replying to Mr Walton's Full Answer in a Letter to P. T. P.*
- 1735-7 *The Querist, containing several queries, proposed to the consideration of the public. Part I, 1735. Part II, 1736. Part III, 1737*
- 1737 *Queries Relating to a National Bank, Extracted from The Querist. Also the Letter Containing a Plan or Sketch of such a Bank*

Whatever aspects of Berkeley's philosophy these works touched on, each bore as a whole on the present field. Dates of first editions only are mentioned. Most of his other published works contain relevant passages. Apart from some remarks about number, nothing will be found in these passages that is not to be found above, though some of them contain a more striking expression of his views. The passages are as follows (short titles only being given) :

## AN OUTLINE OF BERKELEY'S LIFE

### RELEVANT PASSAGES

- 1710 *The Principles of Human Knowledge*  
 Philosophy of science, §§ 30-32, 62, 65, 97-99, 101-117  
 Philosophy of mathematics, §§ 118-132
- 1721 *An Essay towards Preventing the Ruine of Great Britain*  
 Economics, *passim*
- 1732 *Alciphron*  
 Psychology of Vision, Dial. iv, §§ 8-12  
 Philosophy of science, Dial. vii, §§ 4-7, 11-13  
 Philosophy of mathematics, Dial. vii, §§ 4, 5, 11-14
- 1744 *Siris*  
 Philosophy of science, §§ 231-252, 254, 258, 264, 266, 270, 271, 291-296  
 Philosophy of mathematics, § 288

It should be mentioned that the classification made here and the selection of passages must inevitably be somewhat arbitrary. Moreover *Siris* could have been given in the main list as devoted to natural science and medicine, but the interpretation taken here is that it consists of pseudo-science.

### POSTHUMOUSLY PUBLISHED

*Of Infinities.* Written about 1706

*Philosophical Commentaries.* Notebooks of fundamental importance written between summer 1707 and summer 1708. Not mainly about science or philosophy of science, but contain enough important comments in this field not repeated in the above works to justify inclusion in this bibliography

*The Irish Patriot or Queries upon Queries, whereby it is made manifest that a National Bank is utterly inconsistent with the rights, privileges and interests of Ireland*

London School of Economics and Political Science  
 Houghton Street, Aldwych, London, WC2

## REVIEWS

*A Review of Berkeley's Theory of Vision, Designed to show the Unsoundness of that Celebrated Speculation*, Samuel Bailey (James Ridgway, London, 1842. Pp. iv + 239. Out of print)

*Sight and Touch : An Attempt to Disprove the Received (or Berkeleian) Theory of Vision*, Thomas K. Abbott. Illustrated with woodcuts. (Longman, Green, Longman, Roberts, and Green, London, 1864. Pp. vi + 178. Out of print)

MR BAILEY was the first to challenge the universally accepted theory of vision put forward by Berkeley. No doubt, as has been claimed, he slightly misinterpreted that theory ; but he raised a number of acute objections which would be difficult to answer. Standing on his shoulders, Professor Abbott, a Fellow of Berkeley's own College, raised still further objections. Both works consist of sustained and precise arguments against the received theory and in favour of the commonsense view, Abbott's being slightly the more important. Whether these critics are right or wrong, their arguments cannot be neglected by anyone who is interested in the relation between visual and tactual perception, or who tries to estimate Berkeley's contribution to this subject. In general both books are very clearly written.

According to Berkeley's theory, visual percepts are at no distance from the eye. They are constantly associated with tactual experiences—the sensations in the muscles of the eye, the sensations of movement towards the objects involved. Such associations lead a person to believe he sees distance, whereas in fact he passes from visual percepts to tactually and kinæsthetically experienced distance without realising it. To show this, Abbott points out that four steps have to be established : (1) that sight does *not* perceive distance ; (2) that distance is perceived by touch ; (3) that there exist appropriate associations between visual percepts and actual sensations ; and (4) that variations in the suggesting visual percepts correspond to variations in the distance perceived. Abbott asserts that Berkeley offered but one argument for (1), assumed (2), and overlooked the others. Abbott is right to make these links explicit, even though he exaggerates in saying that Berkeley gave no attention to (4).

The decisive items are (1) and (2), and it is in his handling of these that Abbott is at his best. He claims that sight *does* perceive distance and that touch does *not*. He gives a good discussion of the laws of association and uses the resulting criteria against Berkeley. Among his catalogue of objections are the following :



## REVIEWS

(a) We see a number of objects but none is touched, save the pen, footstool, and chair; and every object is seen perhaps a thousand times without being touched for one occasion of being both seen and touched—surely a poor basis for the required association. Sometimes a person unconsciously brings his hand to his head or his knee. But then the visual percept of the pen and the tactual sensation in his hand are different, and he judges them to be at different distances—and this is the ordinary case. 'It is only in an infinitesimally small number of cases that the eye and hand bring reports simultaneously from the same point. For it must be remembered, that the association must be established separately for every distinguishable adjustment of the eyes, and for every distinguishable amount of muscular exertion, as well as for every degree of brightness and confusion' (p. 20).

(b) Why do we not associate sight with hardness and claim to see hardness as we see distance?

(c) 'When at last we reach the object, the idea or sensation which is really present is not the visual idea which we had when we started, but one which we do not at all want to associate with our carefully acquired perception. . . . We cannot at one and the same moment be looking at an object five, ten, fifty yards off, and be achieving our last step towards it' (p. 24).

(d) Tactual sensations would be more likely to suggest, if an association exists at all, those of sight than *vice versa*.

(e) In the dark, we feel our way, making all kinds of errors about distance that we do not make when we see. Many find it difficult to distinguish a florin and half-crown by touch, while a single glance suffices. It is laborious getting an idea of a thing by touch alone and we usually get an imperfect idea of it. (Some of these points are concerned with size rather than distance. Though for the most part Abbott confined his attack on Berkeley's theory of distance, he was certainly opposed to Berkeley's similar theory of size.)

(f) Arrange peas in a circle, say, or straight line with the eyes closed and you will find a much greater error than when your eyes are open. (This is as much concerned with location as with distance.)

(g) A blind man, it is true, can be remarkably accurate in his perception of distance. This might seem to show that touch provides the information required by Berkeley's theory. But his touch is practised. That is to say, those with normal vision are not practised, and do not make the same use of touch.

(h) After touch establishes that a certain surface, which looks smooth, is in fact rough, it continues to look smooth. After touch establishes that there is no object behind a mirror, a mirror-image continues to be seen at a distance behind the mirror. Thus sight does not learn from touch, as

## REVIEWS

it should on Berkeley's theory. We continue also to see an object near the eye as double. The moon looks small, despite our knowledge to the contrary.

(i) One touch-sensation can be reported where the eye can see two compass points on the skin. Thus touch is less discriminating than sight.

(j) Focusing on objects at different distances would have to await correct judgments of brightness, etc., and this would take time; but focusing is immediate.

(k) The sense of touch provides estimates involving *time* rather than *space*—so fundamentally it is different. (Abbott attributes this point to Platner.)

Bailey, too, made some of these objections. He also made others of which the following are striking examples:

(i) While an orange is being both touched and seen its visual appearance should not suggest anything external; but when the percipient ceases to touch it, its visual appearance should at once make this suggestion. For such a change, however, there is no empirical evidence.

(ii) If we do at all connect visual perceptions and tactual sensations, how much more do we connect visual perceptions with other visual perceptions. In other words, as we move towards an object, what we see leads us to expect not a tactual sensation but another visual perception.

The force of Bailey's own presentation of his objections is somewhat weakened by his tacit assumption (to be found, for instance, on pp. 21-23, 61, 68, 144) that Berkeley was correlating a visual experience of distance with a tactual experience of it; whereas in fact Berkeley was correlating with this latter the visual experience not of distance but of certain signs such as blurring and the like. But his objections can generally be made without making this faulty assumption. Bailey also asserted that Berkeley confused two different questions, one being whether distance could be seen at all, and the other being whether two objects could be seen to be at different distances. He does not substantiate this charge, and it does not seem to me that Berkeley did confuse the questions.

On the positive side both authors maintain that sight has the capacity to perceive distance. Abbott asserts that the only way to refute the common-sense theory would be to show that there is a lack of concomitant variation between distance visually perceived and physical distance (p. 75). This is unconvincing: if the required variation were present it could without straining be interpreted to fit Berkeley's theory; thus Abbott's criterion would not discriminate between the theories.

Abbott also goes into questions of monocular and binocular vision, drawing on a considerable amount of scientific knowledge; and he gives exceptionally good case-histories bearing on Molyneux's famous problem.



## REVIEWS

It is hard to believe that Berkeley's theory can survive the criticisms of these authors. But, if there is something basically wrong with his theory, the question is whether there is or is not also something basically right about it. Here I am concerned only to draw attention to the importance of these works and to describe enough of their contents to show that any attempt to grapple with the problem must take them into account.

J. O. WISDOM

*Berkeley: Philosophical Writings.* Selected and edited by T. E. Jessop (Nelson, Edinburgh, 1952. Pp. xxvi + 278. 10s. 6d.).

THIS is a welcome selection. It contains nearly all of Berkeley's three early classics, about two-thirds of *De Motu*, the second dialogue of *Alciphron*, and a small part of *Siris*. The difficulty of making cuts in the early works is a tribute to their closely knit presentation. *Alciphron* poses a difficult problem to the selector for quite a different reason, and Professor Jessop evidently dealt with it by selecting the part dealing with ethics. The highly important *De Motu* is markedly improved by the editor's blue pencil. The theme of its forceful contributions comes through more clearly as a result of continuity; in the full text it tends to become hidden by Berkeley's dull digressions. The selection from *Siris* most aptly consists mainly of those sections that, like *De Motu*, deal with the philosophy of physics. No two people would agree exactly about what ought to be retained and what omitted. I regret the omission from *De Motu* of the highly significant sections on the two globes and the fixed stars (after all Mach, who does not seem to have known of Berkeley's work, made his reputation on a philosophy which is contained, this point included, in *De Motu*). Possibly a few sections from the *Theory of Vision Vindicated and Explained* would have been appropriate. Still there can be no doubt that the selection has been very judiciously made. The Editor's translation of the *De Motu* selection is highly satisfactory and seems to be the most accurate of the renderings so far made.

Would Professor Jessop consider producing 'A Selection from Berkeley's Writings on Science'? One can imagine an attractive volume containing the *Theory of Vision*, the greater part of *De Motu* and *The Analyst*, and excerpts from the other mathematical writings and from *The Querist*, together with a few relevant passages from the other works.

J. O. WISDOM



## ANNOUNCEMENT

### *Prize Essay on 'The Age of the Universe'*

THE Philosophy of Science Group of the British Society for the History of Science announces an Essay Competition, open to all. A prize of £ 50 is offered for the best Essay of not more than 4,000 words (shorter essays being acceptable) on :

*'What is the logical and scientific status of the concept of the temporal origin and age of the Universe ?'*

(for example, as used in recent cosmological work). The essay should clarify the logical, theoretical, and observational aspects of the idea of assigning a quantitative age to the universe.

Essays in English, French, or German (in typescript) must reach the Hon. Secretary of the Philosophy of Science Group, University College, Gower Street, London, W.C. 1, not later than 1st December 1953, must bear a pseudonym, and must be accompanied by a sealed envelope containing the pseudonym and the author's name.

The Judges will be :

Professor Sir Harold Jeffreys, F.R.S. ;

Professor F. A. Paneth, F.R.S. ;

Professor K. R. Popper ;

Mr L. L. Whyte.

Essays submitted will be treated as material offered to the *British Journal for the Philosophy of Science*. The Prize Essay, together with others of adequate quality, will be published. The right is reserved to divide the Prize, or not to allocate it if no contribution of sufficient worth is received.

The Committee wish to thank Mr Karl Bostrom of Milwaukee for a very generous contribution towards the prize money.